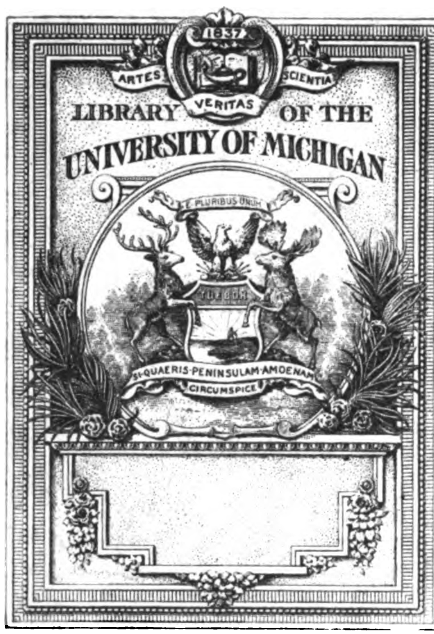
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THE

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EDITED BY

G. STANLEY HALL,

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VESTIGIAL INSTINCTS IN INSECTS AND OTHER ANIMALS[1]

By William Morton Wheeler

The once widely accepted notion of instinct activities as fixed and immutable processes of inscrutable origin has been definitively discarded and replaced by the view that they, like the organism of which they are an expression, are capable of a considerable degree of individual fluctuation or variability. As James[2] says: "In the instincts of mammals, and even of lower creatures, the uniformity and infallibility, which a generation ago were considered as essential characters, do not exist. The minute study of recent years has found continuity, transition, variation and mistake, wherever it has looked for them, and decided that what is called an instinct is usually only a tendency to act in a way of which the *average* is pretty constant, but which need not be mathematically true." Similar statements have been made by other authors imbued with the importance of the genetic study of animal behavior. H. E. Ziegler says:[3] "All the principles that have been established for the morphological consideration of organs hold good also for the instincts; when we refer to them, we also speak of homology, analogy and parallel development, of individual variation, of natural selection and its purposeful result, of artificial selection and hybridization, of becoming vestigial, of re-

[1] Read before the section of Psychology and Anthropology of the New York Academy of Sciences, Nov. 25, 1907.

[2] The Principles of Psychology, II, New York, Henry Holt, 1890, p. 391, *nota*.

[3] Ueber den Begriff des Instincts. Verhandl. deutsch. zool. Gesell. Leipzig, 1891, p. 134 *et seq*.

version (atavism); both here and there we recognize instances of inhibition of development, of natural and artificial abnormalities." As a rule, however, writers have been most concerned with the development or evolution of instinct activities, and have said little or nothing about their involution or disappearance. Permit me for the present to leave the deadlock of opinion on the origin of instincts, as I have no means of deciding whether these are inherited, mechanized, individually acquired activities as claimed by the Neolamarckians, or spontaneous, congenital mutations as claimed by the Neodarwinians, and let us consider some of the cases of their gradual waning and evanescence.

Darwin seems to have been the first clearly to recognize certain animal instincts as purposeless relicts of once highly purposeful adaptations. He dwells on these cases in connection with the expression of the emotions in man and the effects of domestication on animals. G. H. Schneider summarizes Darwin's observations in connection with his own remarks on vanishing instincts, as follows:[1] "Just as the tenacity of heredity accounts for the transmission of organs which have been functionless and vestigial for generations, so it also transmits vestigial impulses (Triebe), or relations between cognitive acts and impulses, which are now purposeless. I allude especially to the oft-cited case of the dog, which, just before lying down, often turns for some time in a circle, even when it is in a room, and not, like its feral ancestors, in the wilds where this gyration is executed for the purpose of treading down the grass. I would further call to mind certain well-known motions of house-dogs and house-cats, which like their feral allies, try to cover their excrement, even in places where there is no sand. These particulars have been far too little observed in animals, or we should undoubtedly be able to cite many other similar examples. Man, too, is known to have such vestigial impulses. The most familiar of them are what I have called the movements of intimidation. Although in civilized man the instinctive uncovering of the canine tooth in the expresssion of contempt, or of all the teeth in rage, has no longer the purpose of intimidating the enemy by a display of weapons, this habit has nevertheless been retained down to the present time. Similarly the aspirated interjection often uttered in a fit of rage, and so like the 'spitting' of the carnivores, is now purposeless, though the impulse to this form of expression is still generally inherited."

The examples of vestigial instincts cited by Darwin and Schneider all agree in conforming to the definition of instinct

[1] Der Thierische Wille: Leipzig, Ambr. Abel, 1880, p. 418, 419.

as an action performed by all the individuals of a species in a similar manner under like conditions. There is, however, another category of these vestiges which seems to represent a more advanced stage of decay or evanescence, in that they seem to be performed only by certain individuals of the species, in circumscribed portions of its geographical range, and only under very unusual stimuli. In my study of the ants, I have repeatedly come upon instinct vestiges of this character, remains of activities that must once have been of the greatest importance to the species, but have since fallen into desuetude and been overlaid or all but completely replaced by more recently acquired instincts. Cases of this description are most obvious in the parasitic species or in those that have changed their nesting habits within comparatively recent times. Forel has called attention to vestigial slave-making instincts in *Strongylognathus huberi* and *rehbinderi*,[1] ants now living as abject parasites in the nests of *Tetramorium cæspitum*, but in all probability descended from slave-makers like *Polyergus rufescens*, which they still resemble in the peculiar falcate structure of their jaws. *P. rufescens*, too, has its vestigial instincts. The workers of this species are no longer able to take food except from the tongues of their slaves, and perish when these attendants are removed, but the queens have retained to a very slight degree the ability to feed independently. This case, and many others that might be cited, are interesting as proving that the castes in polymorphic insects may show different stages in the decay of the same instinct, a condition obviously correlated with visible differences in physical organization and dependent in ultimate analysis on the physiological division of labor so beautifully developed in these and all other social insects. In other words, we not only find the ants exhibiting vestigial instincts as species, but a certain caste within the species may show vestiges of instincts whose full exercise is the normal prerogative of a different caste. Thus under extraordinary circumstances, the usually sterile worker may lay eggs, like the female, or the female may occasionally forage like the workers or accompany them on their slave-making expeditions. In *Leptothorax emersoni*, an ant that lives with *Myrmica canadensis*, I have observed an even more striking example of an obsolescent feeding instinct. The *Leptothorax* when living with the *Myrmica*—and in a state of nature it is never found except in this association—always obtains its food either from the tongues of its hosts (*i. e.*, by regurgitation), or by licking their oily bodies, but when it is

[1]Strongylognathus Huberi et voisins. Bull. Soc. Ent. Suisse X, 7, 1900, pp. 273-280; Miscellanea Myrmécologiques, Rev. Suisse Zool. XII, 1904, p. 2.

separated from the *Myrmica* in an artificial nest it begins to visit the food dish and feeds, rather awkwardly at first, but eventually quite like the non-parasitic species. In this case an instinct, which would certainly be put down by the casual observer as completely absent, can be resuscitated under the conditions of an artificial experiment.[1]

The nidification of ants and other social insects furnishes several examples of vestigial activities, one of which I have described in detail in a former article.[2] *Cremastogaster lineolata*, a common North American ant, which nests in the ground or in rotting wood, belongs to a largely tropical, arboreal genus, many species of which construct great paper or carton nests, roughly resembling the nests of certain social wasps. On very rare occasions, and in a few localities, however, *C. lineolata*, constructs small carton nests or diminutive "sheds" of the same material over the plant-lice and mealy-bugs on whose saccharine excrement it feeds. This is obviously a feeble reminiscence of formerly well-developed carton-building instincts.

I will cite three other cases of vestigial instincts of nidification. The common honey bee (*Apis mellifera*), which in a state of domestication prefers to suspend its waxen combs in closed hives, readily becomes feral and then nearly always takes up its abode in hollow tree trunks. On very rare occasions, however, it suspends its combs in exposed situations from the branches of trees. The form and position of these combs are so much like those of certain species of *Apis* (*A. dorsata* and *florea*) in southern Asia, where the honey bee lived before it was domesticated and exported to temperate regions, that we are justified in interpreting this very unusual method of nidification as a return to ancestral conditions; in other words, as a revival of a lingering or vestigial instinct, called forth by some unusual stimulus, such as the inability to find a suitable nesting cavity at the proper time and in the proper place. The manifestation of this instinct in temperate regions is worse than purposeless, for it leads to the extinction of the colony on the approach of winter.

This case of the honey bee leads me to the consideration of another comb-building insect, one of our common wasps (*Polistes metrica*), which is also of tropical origin. It has, how-

[1] For an account of the symbiosis of these ants see my papers: The Compound and Mixed Nests of American Ants. Amer. Natur. XXXV, 1901, pp. 431-448; Ethological Observations on an American Ant (Leptothorax Emersoni Wheeler), Arch. f. Psychol. u. Neurol. II, 1903, pp. 1-31, 1 Fig: and Notes on a New Guest-Ant, Leptothorax glacialis, and the Varieties of Myrmica brevinodis Emery. Bull. Wis. Nat. Hist. Soc. V, 1907. pp. 70-83.

[2] The Habits of the Tent-building Ant (Cremastogaster lineolata Say). Bull. Am. Mus. Nat. Hist. XXII, 1906, pp. 1-18. pll. I-VI.

ever, migrated of its own accord into northern regions and has acquired a new series of instincts in adaptation to the great changes of the seasons and the food supply. This insect builds small paper combs and feeds its larvæ on the nectar of flowers and fragments of insects. It does not, as a rule, store up these substances, but uses all the cells of its comb for breeding purposes. In the autumn the colony perishes, with the exception of one or a few fertilized females, which hibernate under the bark of trees, in barns or in the attics of houses and start fresh nests and broods during the ensuing spring. The following observations show that *P. metrica* and some other species of the same genus may exhibit vestiges of instincts once highly developed in their ancestral tropical environment.

November 3, while walking with Professor J. M. Cattell over his estate at Garrison-on-Hudson, I found a number of small nests of *Polistes metrica* hanging from the eaves of a boat-house. These nests were empty and abandoned by the insects, with the exception of one, about 5 cm. in diameter and consisting of a few dozen cells. Four female *Polistes*, inactive with the cold, were clinging to the comb, six of the cells of which contained half grown, much contracted, but still living larvæ. There were small drops of a colorless liquid in many of the other cells and one of them was half full of this substance, which was tasted and proved to be honey of an agreeable flavor. The drops hung suspended in the angles of the cells, but were without any definite arrangement and varied much in size. This honey must have been collected some weeks previously from the autumn flowers and stored, now that nectar and insect food were no longer to be had, for the purpose of bringing the few remaining larvæ to maturity. This belated brood undoubtedly accounted for the presence of the female insects at so advanced a date.

I doubt not that other entomologists have noticed this tendency of our species of *Polistes* to store honey, but the habit is certainly very infrequent. Among the observers who have studied European species of *Polistes* most closely, von Siebold and Ed. André have made no mention of this peculiar instinct. It has, however, been noticed by Lepeletier,[1] Rouget[2] and Marchal.[3] Lepeletier says concerning *Polistes gallica* that "at the time when the comb, in process of construction, contains cells suitable for the education of the males and fertile females, the worker *Polistes* begin to collect the provisions of honey that seem to be necessary for the preparation of the food which is to

[1] Histoire Naturelle des Insects Hyménoptères, I, 1836, p. 496.
[2] Sur les Coleoptères Parasites des Vespides, Dijon, 1837, p. 37.
[3] Observations sur les Polistes. Bull. Soc. Zool. France, XXI, 1896, pp. 15-21, 2 figg.

develop the reproductive powers of these individuals." Rouget, who kept *Polistes* colonies in captivity, found that in the beginning of October, when most of the cells were empty, the insects filled them with a kind of honey, which resembled in color the unrefined sugar with which they had been fed. More explicit observations have been published by Marchal from whose paper the above references are taken. He describes as follows several nests of *P. gallica* var. *diadema* sent him from Lot-et-Garonne, France: "Fifteen nests were sent by my correspondent, the smallest exhibiting 6, the largest 59 cells; all containing as yet only eggs or very young larvæ. Six of them had 5 to 8 cells containing honey. These were situated in the portion of the comb furthest from the pedicel, and the honey was placed on the wall nearest the periphery, about half way between the bottom and the free border of the cell, in the form of a large, colorless drop, adhering to the alveolar wall, and of syrupy consistency and beautiful transparency.

"Microscopic examination revealed in it the existence of a few pollen grains, various foreign bodies, such as certain curved, refractive filaments whose nature could not be determined, spores of rust, the scales of a Lepidopteron, etc., but these bodies were present in small quantity and did not interfere with the transparency of the honey. Its flavor was saccharine and very agreeable. For the most part, the nests containing the honey had reached a stage with more than 30 cells (the smallest had 25, the largest 59); all the nests without honey had 30 cells or less.

"The honey, although generally occurring in cells already furnished with an egg, must evidently represent a provision; for it was precisely the oldest cells, and those nearest the pedicel, containing recently hatched larvæ or eggs ready to hatch, that were destitute of honey. This honey is not, therefore, for the nourishment of the larvæ in the cells in which it is placed, but is simply stored up to be again removed and distributed among the larval colony. I found honey in some cells that contained no eggs or in which the egg had been destroyed or had shriveled to a mere pellicle. This fact seems to point to a beginning specialization on the part of the queen *Polistes* in establishing cells for provisions, but most often, as has been stated, the egg remained intact above the provisions.

"It follows from the preceding observations that *Polistes* is able to collect honey *from the very beginning of founding its nest*, when the queen alone exists. This fact therefore completely precludes the supposition [of Lepeletier] according to which honey is collected by the wasps for the production of the fertile females. All the larvæ of the nest receive nourish-

ment made of the same elements and of a mixed character, that is, both animal and vegetable."

The storing of honey by *P. americanus* in Lower California was recorded several years ago by Brongniart.[1] He says that the nest of this species "always contains larvæ and nymphs, but only during the winter are the cells of the median portion of the comb filled with pale yellow, transparent honey." Some of this honey was sent to the chemist Bertrand, who analyzed it with interesting results.[2] It is known that in general the sweet substance collected by honey bees is composed of almost pure saccharose. This sugar is split up in the proventriculus of the insect into a mixture of dextrose and levulose. It is this mixture, sometimes called invert sugar, accompanied by a small residue of unaltered saccharose, that constitutes ordinary honey. It turns the plane of polarized light to the left. The honey of *Polistes*, on the contrary, turns it to the right, and seems to consist exclusively of dextrose and saccharose without levulose." Bertrand infers from this fact that *Polistes* either does not, like the bees, alter the constitution of the nectar which it collects, or obtains its honey from sources not exploited by the bee. He is inclined to accept the latter alternative, because the honey he analyzed had been stored during the winter. He loses sight of the fact that in Lower California there are always some flowers in bloom during the winter. It seems more probable, therefore, that *Polistes* merely regurgitates the collected nectar into its cells without being able to alter its chemical constitution.

For further light on the instincts of *Polistes* we have to turn to the tropics, especially to America, where we find the metropolis of this and the allied genera of social wasps. Here, in contrast with the single genus *Polistes* represented by a very small number of species in Europe and the United States, we find according to the recent studies of R. von Ihering[3] and Ducke,[4] no less than eighteen genera (*Melissaia* [=*Nectarina*, *Caba*], *Pseudochartergus*, *Chartergus*, *Clypearia*, *Charterginus*, *Parachartergus*, *Polistes*, *Monocanthocnemis*, *Mischocyttaris*, *Synœca*, *Synœcoides*, *Tatua*, *Apoica*, *Leipomeles*, *Polybia*, *Metapolybia*, *Protopolybia*, and *Megacanthopus*), with more than a hundred described species from Brazil alone. This series of

[1] Note sur les Hyménoptères du Genre Polistes recueillis par M. Diguet en Basse-Californie. Bull. Mus. d'Hist. Nat., 1895, p. 37, 38.

[2] Examen du Miel Produit par une Poliste de Basse-Californie. Bull. Mus. d'Hist. Nat., 1895, pp. 38, 39.

[3] As Vespas Sociaes do Brazil: Revista do Museu Paulista, VI, 1904, pp. 97-309, pll. III-VII.

[4] Sobre as Vespidas Sociaes do Pará. Bol. Museu Goeldi., III, 1905, pp. 317-374, pll. I, II.; Prim. Supplemento *ibid.*, IV, 1906, pp. 652-698, pll. I-IV.

forms embraces 17 species of *Polistes* and 36 species of the closely allied genus *Polybia*. All of these insects construct nests with paper combs, and species of several of the genera have long been known to provision these with considerable quantities of honey. Both H.[1] and R. von Ihering[2] have contributed some valuable observations on this and other habits in these insects. Very little honey and that only in drops is stored up by the Brazilian species of *Mischocyttaris* and *Polistes*, but *Melissaia mellifica* provisions its combs with large quantities of this substance. This is true also of *Polybia sericea*, *sylveiræ*, *edula*, *occidentalis* and *lechuguana*.

I find in the American Museum of Natural History a large nest of the Mexican *P. occidentalis pygmæa*, donated to the institution by Mr. William Schauss. It is subglobular, about 20 cm. in diameter, of a light ochre brown color and attached to the small branches of a tree. It contains several combs placed one above the other, each comprising hundreds of cells 2.5 mm. in diameter and about 5 mm. deep. The lowermost combs have been removed and many of the upper combs cannot be seen without destroying the outer paper envelope, but each of the visible combs has the cells over a large central area filled with nearly mature larvæ and pupæ. Around these there is a zone of empty cells and then follows, next to the wall of the nest, a zone 2-3 cm. broad of cells filled with a dark brown, inspissated honey of agreeable flavor. According to Mr. Wm. Beutenmüller, the lowermost comb, when removed some time ago, contained no brood but was filled with honey even in the centre. This nest therefore resembles a beehive in having the brood cells in the centre both of the individual combs and of the series of combs, and the honey cells on the periphery.

The honey of the South American wasps seems to be highly toxic, at least in certain localities or in certain seasons. Azara[3] describes cases of poisoning from the honey of the tatú wasp (*Tatua morio*, according to H. von Ihering) and Auguste de St. Hilaire[4] gives a vivid account of the effect on himself and two companions of eating the honey of the lechuguana wasp (*Polybia lechuguana*) in Uruguay. On this occasion the honey produced pain in the stomach and intense cerebral excitement followed by drowsiness and excessive debility.

The great quantities of honey collected by the tropical wasps are, of course, stores of provisions for the winter, for, as the von

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[1] Biologie der stachellosen Honigbienen Brasiliens. Zool. Jahrb. Abth. f. System. XIX, 1903, pp. 179-287, pll. X-XXII, 8 textfigg.

[2] *Loco citato*.

[3] Voyages dans l'Amérique Méridionale, V, 1, 1809, p. 160.

[4] Relation d'un Empoisonnement causé par le miel de la Guèpe Lecheguana. Ann. Sci. Nat., IV, 1825. pp. 340-344.

Iherings have shown, many of the species, unlike the northern *Polistes*, do not abandon their nests on the approach of the unfavorable season and start new ones in the spring, but continue to add to their combs and keep on raising their brood throughout the year. These naturalists are unquestionably right in deriving the conditions seen in our northern *Polistes* from those of the tropical species. There can be little doubt that *Polistes* has extended its range into North America and Europe since the close of the glacial epoch. The storing of honey for the winter has been discontinued and the life of the species has been saved by a new set of adaptions involving the abandonment of the nest, the temporary suspension of the breeding instincts and the hibernation of a small number of fertilized females. The drops of honey occasionally stored in the nests are all that remains to point to a once very important means of tiding over the flowerless season and preserving the life of the individual colony. Rouget, Brongniart and myself have observed this vestigial instinct only in the autumn, and this would seem to be the most likely time for a feeble display of the old habit. Marchal observed it in the spring. In this case the instinct may have been resuscitated by a protracted spell of cold weather, scarcity of flowers or other conditions simulating the oncoming of winter.

Not only has the honey-storing instinct of our northern *Polistes* been reduced to a feeble and useless vestige by the adaptation of this insect to life in a temperate zone, but the nest-building instincts, when compared with those of the allied tropical wasps, show unmistakable signs of a similar degeneration. In *Polybia* and several other genera the nest consists of a number of combs, each comprising hundreds of cells and the whole is enveloped in a paper involucre as in the circumboreal genus *Vespa*, but in *Polistes* the involucre is no longer constructed and the nest has dwindled to a single comb with comparatively few cells. And this reduction in the size of the nest has led to, or is the result of, a reduction in the size of the colony.

Non-social insects also occasionally exhibit vestigial instincts of nidification. Schröder[1] has recently obtained experimental proof of the existence of these in the caterpillars of a Tineid moth (*Gracilaria stigmatella*), an insect which feeds on the willow and for this purpose conceals itself in a little case made by folding the point of the willow leaf over onto the under side, curling it up and closing any openings with silk. Schröder reared a brood of caterpillars and placed them on willows with

[1] Ueber experimentell erzielte Instinkt-variationen. Verhand. deutsch. Zool. Gesell. 1903. pp. 158-166.

the tips of the leaves cut off or on trees with leaves of a different shape. He found that 84 out of 91 individuals thus deprived of the opportunity of constructing their cases in the normal manner, built cases by rolling up the edges of the leaves. A second generation bred from these insects on leaves with their tips cut off, behaved in the same manner. A third generation was then bred on uncut leaves and of the 19 cases constructed 15 were of the typical or normal form (*i. e.*, with the tip of the leaf folded over), but four were made by rolling up one or both edges of the leaf. From these experiments Schröder concludes that "when the spinning glands are exhausted or when there is no opportunity to construct the case in the typical manner, phylogenetically older instincts, which are still manifested by other species of the genus, are released. Even still older instincts, in fact, the most primitive instincts of the microlepidopterous larvæ, may manifest themselves, as when the larvæ merely conceal themselves between leaves loosely drawn together. Such modified instincts may be repeated by the offspring without a repetition of the stimulus which first caused their appearance."

In the following example we see another simple instinct emerging under very definite conditions that closely simulate the conditions under which the creature's ancestors once lived. The common pond-snails of the genus *Lymnæus* live in shallow water and breathe atmospheric air by means of a lung, *i. e.*, a sac whose walls are lined with blood-vessels. And although these snails are able to take up directly through their integument the oxygen dissolved in the water, they are nevertheless compelled to rise to the surface and renew the air in their lungs at intervals varying from fifteen seconds to several hours, except during the first days of their lives, when they obtain all their oxygen directly from the water.[1] Many years ago F. A. Forel,[2] von Siebold[3] and Pauly[4] found living at the bottom of lakes in Bavaria and Switzerland certain *Lymnæi* that could not, for very obvious reasons, come to the surface to breathe. Forel noticed that snails brought up from a depth of 250 m. in Lakes Leman and Constance had their lungs filled with water, showing that these animals had long since aban-

[1] Walter, The Behavior of the Pond Snail Lymnæus elodes Say. Cold Spring Harbor Monographs VI, March, 1906. pp. 35.

[2] Introduction à l'étude de la faune profonde du lac Léman. Bull. Soc. Vaud. Sci. Nat. X, No. 62, 1869, and Materiaux pour servir à l'étude de la faune profonde du lac Léman. *Ibid.* XIII, No. 72, 1874.

[3] Ueber das Anpassungsvermögen der mit Lungen athmenden Süsswasser-mollusken. Sitzb. math. phys. Cl. K. Akad. Wiss. München 1875, Heft I.

[4] Ueber die Wasserathmung der Limnæiden. Gekrönte Preisschrift. München 1877, pp. 47.

doned the typical pulmonate method of respiration. But when these same snails were placed in shallow water they repeatedly rose to the surface and filled their lungs with air, like the common pond snails. Similar observations were made by Pauly on the snails of Lake Ferchen. These, in fact, definitively abandoned the habit of breathing water with their lungs after they had once had an opportunity to reach the surface. Here we have a sudden adaptation to new conditions in the life of the animal through the artificial resuscitation of an instinct that for generations has been kept in abeyance, an instinct which might properly be designated as vestigial if it were not so perfectly manifested.

A search through the extensive literature of animal behavior would probably yield many additional examples of obsolescent instincts, but the foregoing will suffice for present purposes. They point to the following conclusions, which, I believe, should not be overlooked in the study of comparative psychology.

1. The vestigial instinct action presents itself as an act of racial or phyletic recollection (Mneme, in the sense of Semon[1]) and must, like the representations of individual memory, depend on psychophysical dispositions abiding in latency, just as the visible morphological characters of the adult organism arise from invisible physiological dispositions in the germ-plasm. These dispositions must be inherited with great tenacity and persistency, since vestiges, both instinctive and structural, often remain latent for generations and then suddenly manifest themselves under the stress of extraordinary stimuli.[2] I am not here concerned with the nature of these dispositions in themselves, for they belong to the category of hypothetical postulates or constructions and at present answer their purpose if they enable us to subsume such apparently disparate phenomena as heredity, racial and individual memory under a common point of view.

2. The vestigial instincts obviously represent a part of the

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[1] Die Mneme als erhaltendes Princip in Wechsel des organischen Geschehens. Leipzig, Wilh. Engelmann, 1894.

[2] Many of these vestigial manifestations may therefore be regarded as cases of atavism, when they fail to reappear in all generations, but I have avoided the use of this term, because, as Emery has shown (Was ist Atavismus? Verhandl. V. Internat. Zool. Congress. Berlin, 1901 [1902]) it may be understood in two different senses, either as the reappearance by discontinuous heredity of peculiarities belonging to remote ancestors, or as a special potency or faculty which brings about such reappearance. There can, of course, be no objection to the use of the term in the former sense, but its employment in the latter has had a tendency to discredit the whole subject of discontinuous or latent heredity.

animals' endowment, and their manifestation shows that the capacities of even the lower organisms are greater than their ordinary routine behavior might lead us to suppose. I am not aware that any teleological interpretation has been offered for the marvellous persistency of vestigial instincts and structures. At first sight such a conception would seem to involve a contradiction, since it is usually supposed that these vestiges are functionless and purposeless. If all we mean by these words is being no longer able to respond adaptively to a particular set of conditions, the vestigial organ or instinct may indeed be said to conform to this definition. In this sense the honey storing of *Polistes metrica* as an instinctive response to the cold of autumn, is quite purposeless. But there is another and broader sense, in which such vestiges assume a very different dignity. The organism can exist only in a cosmic setting, and this setting is continually and sometimes suddenly changing. Conditions that have once existed may and often do recur after long lapses of time, or what may amount to the same thing, the organism may migrate or extend its range into regions like those inhabited by its remote ancestors. When this occurs a stock of instinct relicts may be of the greatest utility to the organism, for the persistent inheritance of enfeebled tendencies and impulses may then make possible rapid readjustments to the new conditions. For example, if our winters should become milder, or if *Polistes* should extend its range to such subtropical islands as the Bermudas or the Azores, a manifestation of the latent and enfeebled honey-storing instinct might acquire the value of an incipient adaptation and enable the species to survive under conditions unfavorable to hibernation. Similarly, if the Swiss lakes should become shallow through sudden upheaval of their floors, the deep water *Lymnæi* could at once revert to the respiratory habits of their paludicolous ancestors and, should the desiccation continue, they might even become terrestrial like the land Pulmonates. This last possibility suggests that even when conditions merely change without recurring, vestigial organs and instincts may be useful as starting points for entirely new adaptations, for there are not wanting cases of vestiges that have acquired new functions. In our common Dipterous flies, *e. g.*, the halteres, or vestiges of the posterior wings have become sense-organs since they ceased to reinforce the action of the anterior wings in flight; and the vermiform appendix of man is said to have acquired the function of an intestinal tonsil.

3. The foregoing and similar considerations lead me, in conclusion, to a few remarks on the method of studying vestigial instincts and instincts in general. Some Germans and their over-zealous followers in other lands, have come to look

with a certain disdain on all methods of biological research not strictly experimental. I am, of course, willing to concede many of the claims of these authors. It is evident that in all cases like those above cited, vestigial instincts become manifest through the incidence of unusual conditions. When such conditions intervene in the natural environment of an organism, we have one of nature's experiments, when they are devised and applied by the investigator we have the laboratory experiment, but in both cases the emerging instinct can be correctly understood only by an application of the comparative and historical methods. Thus, as we have seen, the honey-storing instinct of our northern *Polistes* cannot be satisfactorily interpreted without recourse to a study of the whole genus or family to which the insect belongs, and a comparison of the instincts displayed by the various species in their normal environment. And the sudden change in the respiratory instincts of the deep water *Lymnæi* is quite incomprehensible without a reference to the habits of the shallow water and terrestrial Pulmonates. The necessity of applying the comparative and historical methods is also shown in many negative cases. When we ask, *e. g.*, why our domestic pigeons no longer alight or nest in trees, or do so only in very rare instances, no amount of experimentation on the nesting habits of these birds can assist us in answering this question. A comparative study, however, shows that the domestic bird is in all probability derived from the rock-pigeon, a form that had developed an instinct to alight and nest only on cliffs or open ground, and this peculiarity, granting the wonderful conservatism and other peculiarities of hereditary transmission, accounts for the negative behavior above mentioned. Similarly, an explanation of the remarkably small size of the colonies of the northern *Polistes*, as contrasted with the populous communities of the tropical *Polybia* and the allied genera, can be obtained only from a comparative phylogenetic study of the various social wasps with reference to their present natural environments. The tendency of some of our workers in animal behavior to pick and choose single, convenient forms for study—a legacy of the morphological regime which has held sway in our biological laboratories—is therefore unfortunate to the extent that it narrows the field of inquiry of the individual investigator. I am convinced that our knowledge of many of the aspects of instinct, like the one I have been considering, would gain immensely by the comparative study of whole genera or families of closely related organisms, for we know of no case in which an instinct is peculiar to a single species (unless it be, perhaps, the human instinct of reason), and of no cases in which two species manifest an instinct in precisely the same manner.

ATTENTION AND INTEREST[1]

By William H. Burnham

It was said by the older psychologists, and is repeated to-day, that attention depends upon interest. Nobody denies the truth of this statement, but it is a survival of a psychology fast becoming obsolete. It is equally true and quite as important that interest depends upon attention. The word interest, as everybody knows, is used in two senses:—first, as practically all psychologists agree, it denotes a complex state of feeling; second, it denotes a permanent mental possession—as when we say a man has an interest in art, or literature, or music, or the like. What is meant is that one has a store of associations related to these subjects, that he is in the habit of attending to them; or better, as an essential element in attention is preperception, that one has a permanent habit of preperception in such subjects. These two meanings of the word interest are as different as they can well be—the one a temporary affective state, the other a permanent habit of preperception. But, as it seems, even in our standard psychologies these two uses of the word are often confused. Fortunately, perhaps, for the preservation of my friendships time is lacking for concrete illustration.

If the word interest is used in the latter sense, to signify a permanent habit of preperception, nobody will deny that attention depends upon interest; but this is merely saying that our present preperception depends upon our past habits of preperception. If, however, by interest is meant the affective state, it is not in harmony with modern psychology to say that attention depends upon interest. Psychologists are coming apparently more and more to the view that every reaction of the organism contributes its increment, however infinitesimal, to the affective life of the individual. The organic adjustments in the case of attention are no exception. They contribute their increment of feeling. The resulting affective state is interest.

All writers are agreed that physical reactions of varied character are the necessary conditions of attention; all agree that interest is an affective state. Modern investigation has amply demonstrated the physiological conditions of attention, sensory, motor, vasomotor, circulatory, respiratory, visceral. Why

[1] This paper is printed substantially as presented at the Cambridge meeting of the American Psychological Association, Dec., 1905.

should not this organic adjustment be correlated with an affective state of consciousness? What is this affective state but interest? The theory I would present is simply this: The feeling of the organic adjustment in attention is the interest. Attention is a fundamental reaction of the whole organism, comparable to the tropisms of plants and animals. As Professor Royce puts it: "Whoever is persistently attentive is expressing an attitude of the organism which has the essential character of the now frequently mentioned tropisms.[1]" The correlative of this is interest. Whenever we turn attention to a subject, we have a feeling called interest. While it might be vain to attempt to show the primacy of one or the other of these correlatives, the facts, in the writer's opinion, are described more accurately by saying that interest depends upon attention than by saying that attention depends upon interest.

This, it may be said, is a mere theory, what is there to demonstrate it? This is a fair question. The answer is that it cannot be demonstrated. But what is the opposite doctrine but a mere theory? It has been assumed, but no one has attempted to demonstrate it. If the primacy of either is to be asserted, the presumption is in favor of the theory that interest depends upon attention. Why should interest stand alone without physical cause, when other affective states are correlated with physical reactions? On the other hand, why should not the organic changes involved, as everybody knows, in attention, the so-called conditions of attention, have their mental correlative in some such affective state as interest? Stated in this way the theory suggested can be held by psychologists with widely different views of feeling. Stated as before,—the feeling of the organic adjustment is the interest,—it is practically a corollary from the Lange-James theory of emotion. In my opinion quite as good a case can be made for the latter as applied to the subtle feeling of interest as in the case of the coarser emotions.

This view of interest has been approximated or anticipated by a number of psychologists, among them, if I mistake not, Prof. Titchener, and Prof. Dewey, and of course by all those who identify attention and interest. Of the last mentioned theory Miss Calkins has given an excellent statement. She rightly says that a thing attended to is interesting, and she makes interest and attention synonymous. We may, I think, carry our psychological analysis a little farther. Even "what God has joined together" psychology is sometimes justified in separating. The relation of attention to interest is such a case. And if for no better reason than to protest against the

[1]Outlines of Psychology, p. 329.

prevailing confusion of the psychology of attention, it would be well to emphasize the organic adjustment as the basis of interest and feeling, and attention as primary.[1]

With this view of interest, the traditional conundrums are explained quite as well as by any other. The pages of the older psychologies were enlivened by thrilling stories of certain orators who, in the passion of eloquent gesticulation, severely wounded their hands on desk or pulpit, but remained unconscious of the fact until their effort was over. The theory here suggested, together with the well-known law of the relativity of feeling, offers a plausible enough explanation of such cases. The intense interest caused by the orator's intense attention so filled consciousness that the pain was excluded; the interest was the stronger affective state. Afterwards, when the orator's intense attention ceased, the interest ceased also, and the pain of the wound was felt.

It has been something of a psychological puzzle that attention weakens feeling. There is no question that it does. If one gives close attention to a pain, for example, the pain is appreciably diminished, and illustrations have been furnished by experiments in the laboratory. Kuelpe states this effect upon the feelings as follows: "While pleasure and pain are brought far more vividly to consciousness by concentration of attention upon their concomitant sensations, they disappear entirely if we succeed (and we can succeed only for a moment) in making the feeling as such the object of attentive observation."[2] Now, if the feeling we call interest is the result of attention, then by the very act of giving attention to a pain we arouse a new feeling that on account of the limitations of consciousness and the law of relativity weakens the former painful feeling; and, if this attention and its concomitant interest are sufficiently intense, they may submerge the pain altogether.

It is often said that interest is contagious. This is a very pretty metaphor and it has long done service in psychology, but no one has isolated the bacillus of interest that causes the contagion, although psychologists seem to have been satisfied with the metaphor. Dropping the figure let us ask how the interest spreads. The following simple hypothesis seems to accord with the facts. One person becomes interested. His

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[1] Since presenting the above paper before the American Psychological Association, the writer has noted the statement by Prof. Pillsbury, which he had not read at the time: "Things are only interesting because we turn our attention to them, but we do not turn our attention to them because they are interesting." But as I understand the context, he does not acknowledge the relation of interest to attention involved in this statement. L'Attention. Paris, 1906. pp. 72.

[2] Psychology, p. 430.

enthusiasm *suggests* in obvious as well as subtle ways that the subject is worth giving attention to. Forthwith others imitate his example and attend to it, and interest follows as the result, first the affective state, then the habit of preperception.

Probably a similar theory accounts in whole or in part for the well-known fact that we are often interested in the disagreeable or the painful; for the spread of fads in a community; for the fact that intense attention often hinders perception and action; for anæsthesia due to hypnotic suggestion, etc.

To résumé briefly: The word interest as we have noted is used in two senses: 1st, to denote our permanent habits of preperception; 2nd, to represent a temporary affective state complex in character. Attention is a reaction of the whole organism, comparable to the tropisms of plants and animals. We must suppose an affective state correlated with this reaction. This affective state is interest. The least we can say in this case is that interest is correlated with attention.

While with legitimate heedlessness we may continue to use the popular phraseology in regard to attention and interest—just as we say the sun rises and sets—in our special psychology of attention it is misleading to teach that attention depends upon interest. If we use the word interest in the sense of a permanent habit of preperception, what we mean is that our attention of the moment as preperception depends on our habits of preperception; but if that is our meaning, we should say so. If, however, we use the word interest in the other sense, meaning the affective state of the moment correlated with attention, to say that attention depends upon interest is not in harmony with modern psychology.

Not only the popular use of the words interest and attention but our psychological usage as well, are misleading survivals of an obsolete psychology. In pedagogy this usage has led to much confusion and error. There are actually two opposing camps—those who believe in the moral value of the uninteresting, and those who believe in the pedagogical value of the interesting. It is said by one side that only by giving attention to the uninteresting can the power of voluntary attention be developed. It is said by the other that only when interest is appealed to can a child's attention be gained. To attempt a reconciliation would be a case of crying peace! peace! when there is no conflict. Both are largely right: for so far as interest means preperception, attention is impossible without it; the child's mind can no more give attention to the absolutely uninteresting in this sense than the eye can perceive the ultraviolet rays of the spectrum; while so far as interest means feeling it is bound to come any way when attention is actually turned to an object.

Of course, any such account of the processes of attention and interest does injustice to the complexity of the actually existing mental states. The process of preperception determines not merely the aspects of an object to which we attend, but to a great extent the intensity and coloring of the affective state, that is, the interest in this sense. That this may occur in a short circuited manner, the preperceptive images being associated with affective states, or if one prefer, they themselves having an emotional coloring, there seems no reason to deny.

The view here presented has been stated with somewhat arbitrary simplicity. If to some the whole matter seems too obvious to be worth presenting, and if to others it seems reactionary, this may be an illustration of what has been said of the prevailing confusion in the use of the word interest.

NOTES ON SOME ASPECTS OF THE FOLK-PSYCHOLOGY OF NIGHT

By ALEXANDER F. CHAMBERLAIN

The author has no intention at present of furnishing a monograph on the psychology of night; he has simply put together some notes on certain aspects of the subject which have seemed to him worthy of more extended investigation by the psychologist, aspects which in previous studies have perhaps not received due attention,—the ethnologic and folk-lore material relating to night and the reactions to it on the part of the various races of man, civilized and uncivilized, contains much of interest and value to the enquirer into the origin and development of human mental phenomena. Certain large sections of the subject such as night and religion, sleep, dreams, fear, the emotions, etc., some of which have been much discussed by other writers, are here but lightly touched upon or reserved for future consideration.

1. *Night and Death.* In the minds of many peoples, civilized and uncivilized, night and death are commonly and inseparably connected,—night, as the time of the absence or the disappearance of light, being conceived of as a period of death, or, at least, of lethal significance. This correlation of life and light, of night and death, is implied in the word *night* itself, which, with its cognates in various European tongues (German Nacht, Latin nox, Greek nux, Sanskrit nakta, etc.), seems connected with certain terms for "death" (*e. g.*, Latin nex; Greek nekus, "corpse;" Sanskrit nac, "to perish"), etc. The common saying "the dead of night" belongs also here, with other kindred expressions. In classical mythology, Nox and her brother Erebus are the children of Chaos, and one of her own offspring is represented as "black death," while she is often referred to as "the mother of death," and likewise of sleep (regarded as the brother of death). But night has also been looked upon as "the mother of man," and "the mother of all," out of whose fertile womb came life itself and all existing things in the universe. That the emphasis upon the lethal character of night and its antagonism to life has been exaggerated, appears from both negative and positive evidence.

Suicide, the most anti-individualistic form of death, seems to prefer the day. Of 8,226 cases, *e. g.*, occurring in the United

States during the six years 1882-1887, the great majority took place in day-time, only 2,419 being recorded at night. Later statistics do not appear to have altered this relation much, if any.

Geck, who has studied the distribution of 100,000 cases of suicide occurring in Germany during the 20-year period 1876-1895, finds an increase paralleling the length of the day, reaching a maximum in June and a minimum in December. The same relation holds if this period is divided into four sections of five years each. A previous investigation by K. Becker, covering 87,439 cases during the period 1872-1885 in Prussia, Saxony, Würtemburg, Baden and Hamburg, confirms Geck's results. Here, too, a division into three shorter periods shows no difference of any significance. Even discounting for the general difference of summer and winter months, and for the increased facilities for self-destruction offered by day-time, there yet remains something in these figures which argues against the theory of the death-influence of night. The same conclusion applies to other forms of death than suicide, similar allowances being made for the industrial, social and other factors more potent during the reign of day.

Statistics of deaths indicate a decided preponderance in day-time. Raseri, whose investigations dealt with 25,474 cases, found the maximum of deaths to occur in the period between two and seven o'clock in the evening, and the minimum during the last hours before midnight.

Night and death have been made much of in literature and art among many peoples, but it was reserved for Joseph Blanco White (d. 1841) in his famous sonnet *Night and Death* to say the genius-word:

> Mysterious Night! when our first parent knew
>   Thee from report divine, and heard thy name,
>   Did he not tremble for this lovely frame,
> This glorious canopy of light and blue?
> Yet 'neath a curtain of translucent dew,
>   Bathed in the rays of the great setting flame,
>   Hesperus with the host of heaven came,
> And lo! Creation widened in man's view.
>   Who could have thought such darkness lay concealed
> Within thy beams, O Sun! or who could find,
>   Whilst flower and leaf and insect stood revealed,
> That to such countless orbs thou mad'st us blind!
>   Why do we then shun death with anxious strife?
>   If Light can thus deceive, wherefore not Life?

The individual bard thus agrees with the folk-poets of many lands in seeing the life that lies in night rather than the death so often inseparably connected with it in human thought. Light and life are thus alike in being born of night.

2. *Night as Mother. Night and life.* Both in the personi-

fication of night in mythology and in the figurative language of poetry the idea of motherhood is prominent. The ancient poets styled her "mother of all things, of gods as well as of men," and in classic mythology she appears as mother of day and light, of the fates and the furies, of the hesperides, of death, sleep and dreams, of discord and strife, of hunger and fear, of nemesis, of satire and fraud, etc. In primitive thought darkness and night, like earth, seem inevitably linked with motherhood. The womb of woman and the womb of night are credited with close and primeval kinship. Milton's figure of "the wide womb of uncreated night" had its savage and barbaric predecessors; so, too, with Whittier's "the night is mother of the day," for, before him, the classic poet had said, "out of night is born day, as a child comes forth from the womb of his mother." Bachofen, the protagonist of the "mother-right" theory in the last century, observed that "motherdom is related to the idea of the day-bearing night, as fatherdom is to light sprung from the union of the sun with mother night." Sometimes the light born of night is so pre-eminent and predominant as to subdue, expel, or destroy all her other evil brood, and we have the undisputed omnipotence of him who is light, and in whom "is no darkness at all."

The folk-idea of "mother" night receives some support from the incidence of human birth, for vital statistics produce not a little evidence in support of the view of Berlinski that night is distinctly favorable to individual life. Birds are said to break from the egg more commonly during the night or early morning. The same is said to be true of birth with many species of animals, and the tendency among human beings seems also to be born at night, or in the early morning. Dr. V. Goehlert's investigation of the hourly distribution of 86,850 births occurring in the Canton of Zürich, Switzerland, during the years 1876-1884, shows a maximum between 12 P. M. and 2 A. M. and a minimum between 12 M. and 2 P. M. The percentages for four-hour periods were as follows: from 12 M. to 4 P. M., 14.33%; 4-8 P. M., 15.01%; 8-12 P. M., 16.56%; 12 P. M.-4 A. M., 19.66%; 4-8 A. M., 18.68%; 8 A. M. to 12 M., 15.76%. Observations in Brussels, Berlin, Hamburg, Dorpat, Edinburgh, at varions dates from 1811 to 1884, substantiate the conclusions of Goehlert. In Dorpat (Livonian Russia) out of 25,382 births during the period 1850-1881, the maximum (40%) for six-hour periods was between 12 P. M. and 6 A. M., and the minimum (12.8%) between 12 M. and 6 P. M. Raseri's examination of the time of birth in 36,515 cases also confirms Goehlert's views, the maximum occurring in the early hours of the morning, the minimum during the first hours of the afternoon. The evolutional defences of life at night are in evidence also, although

not much attention has been paid to them by biologists and students of the animal world. Verrill has recently, however, pointed out some of the nocturnal protective colorations of mammals, birds, fishes, insects, etc. Nature, it seems, shows herself here quite as skillful in protecting her creatures by assimilating their colors and markings to the nightly environment, as she is in the devices she has evolved for ensuring their safety by day.

3. *Night and the dead.* That the spirits of the departed visit at night their former home, gather about the hearth and enjoy themselves in their own way is a folk-belief widespread among the peasantry of Europe,—a belief that has led to many curious and significant customs. Often (as, *e. g.*, in Brittany) the fire must never be allowed to go completely out so that the returning spirits may warm themselves,—or sometimes it is the *Bouffon Noz* (farceur de nuit), or the Virgin herself who comes. And, when the fire has been used to cook the pap for a new-born child, it must be kept up all night so the Virgin may come and get ready the food of the little Jesus. In Brittany the fire is kept up at night by a log called *kef an anaon* ("the log of the dead"). In certain parts of the Vosges country, during the week of All Saints, the windows are opened and the covers of the beds turned down so that the dead may for a brief space sleep in their old place. The philosophy of the "house spirits," and "ancestor worship," looms large here. As day belongs to the living, night belongs to the dead; the bright, crackling fire to men and women of this world, the smouldering embers at least to those who "revisit the glimpses of the moon." This innocuous, friendly and sympathetic side of spirit-visitancy is but one of the attempts made by man to humanize the world beyond the grave and to attach it to himself as part of himself,—to make the immortals mortals again.

After being built, houses are subject to certain ceremonies not alone among savage and barbarous peoples, but also among civilized and Christian nations, with the object of preserving them from the inroads of spirits, especially during the hours of darkness. Similar precautions were and still are taken, in certain parts of the globe against the inroads of the devil and his imps, who preferred the night for their interference with the happiness of men and women. Here belong the harriers of the air, the wind spirits, "wild huntsmen," "night-goers" (with innumerable names), and death, himself, who "rides by night." And against "the pestilence that walketh in darkness," man has continually to guard himself and what is his.

Beautiful, indeed, and thoroughly worthy of the human heart, are some of the household beliefs and practices connected with the "feeding of the dead" and the entertainment in most hos-

pitable fashion of the nightly visitants, who seek so eagerly the scenes of their former existence. Sacred are the thirst and the hunger of the dead; and blessings innumerable flow from their happy spiritual touch. Their gifts are often not to be feared in the least, and their comings and goings are the fortune of their descendants. In what happy touch the living are with the "good dead," one can learn in some region of peasant Europe, a corner of Brittany, for example. The dark side of the picture may be sought in some part of Malaysia, where every soul alive is in endless fear of the roving spirits of the departed.

Against the malevolent spirits of the dead who fill the air of night manifold precautions have to be taken and the ingenuity of man is wearied in efforts to lay these terrors of his environment, who are bold enough sometimes to harrass him even in the broad light of day. For many peoples (not savages and barbarians of the lowest type alone)

> "Millions of spiritual creatures walk the earth
> Unseen, both when we wake and when we sleep."

Not merely this, but in the fearsome night they are seen and terrify the heart and mind and soul of man, till fear itself creates unfear and brings release. But not before innumerable petty and many genial devices have been adopted.

4. *Night as the "evil time."* According to the Andaman Islanders, a very primitive people of Negrito stock, Puluga, the uncreated and immortal creator of all things, who lives in the sky, "is omniscient only while it is day, when he can see." In the darkness of night this faculty leaves him. Where the deities of day and night are not especially arrayed in opposing orders or classes, it often happens that the overruling powers lose wholly or in part their strength during the hours of darkness. This idea finds expression in the saying that "God loses to the devil at night." And in this riot of evil man is often easily involved. The individual and the general thought is well expressed in Hamlet's famous words:

> "'T is now the very witching time of night,
> When churchyards yawn, and hell itself breathes out
> Contagion to this world: now could I drink hot blood,
> And do such bitter business as the day
> Would quake to look on."

And, earlier in the play, the ghost of Hamlet's father, to whom his son had said:

> "Be thou a spirit of health or goblin damn'd,
> Bring with thee airs from heaven, or blasts from hell,
> Be thy intents wicked or charitable,
> Thou comest in such a questionable shape
> That I will speak to thee,"

tells him:

> "I am thy father's spirit,
> Doom'd for a certain time to walk the night,
> And for the day confin'd to fast in fires,
> Till the foul crimes done in my days of nature
> Are burnt and purg'd away."

And the unpublished volumes of the folk-lore of innumerable tribes of man are filled with the record of the words and deeds of the "perturbed" spirits who cannot "rest." And this same literature reveals the existence in times past and in times present of hundreds of "night taboos," prohibitions of acts that, when performed after sunset, or during the hours of darkness, alarm and irritate the spirits of the dead and bring upon the doer their enmity in innumerable ways, stir up to activity the demons of the air, etc., and particularly the prince of darkness, whose "name is legion."

The following, cited by van Gennep, will serve as typical night-taboos (they are from the aborigines of Madagascar):

1. The house must not be swept at night,—that would bring poverty.
2. One must not comb the hair, wash the face, or cut the finger-nails at night.
3. One must not call out, whistle, or carry mutton at night, else the spirits will pursue him.
4. One must not go to bed without having a light in the room,—otherwise he would resemble the dead in the tomb.
5. Ashes or dirty water must not be thrown away at night,—that displeases the spirits much.

It will be seen that the night-lore of these peoples often resembles, or is even identical with, that of our own race. In various parts of France folk-belief forbids sweeping house after sundown, lest some one of the family should die. In the Breton region of the Côtes-du-Nord the reasons assigned are that the souls of the dead who come to visit their old homes during the night are in danger of being swept out with the dust, or the Virgin Mary, who is seeking houses in which to lodge her favorite souls, may be driven away. In some places it is only on the eve of the feast of the dead that this taboo holds,—for fear of driving off the souls of those in purgatory.

Leaving a light burning is not always due, however, to fear of the devil or regard for the spirits, but may sometimes be accounted for from simple economic or social reasons.

Civilized peoples, outside of individual idiosyncrasies, take their sleep in darkness, not leaving lights burning in their bedrooms. This is not, however, the case with all primitive peoples. The Eskimo, notably, as Nansen has remarked, "do not sleep in the dark like other people," the lamps being kept qurning all the time in the *iglu* or snow-hut. This custom,

Hough thinks, may be due in part to their high appreciation of "the feeling of companionship, security or sociability, given by light," and partly also to "the inconvenience of rubbing out fire with the fire-drill to relight the lamps." The long winter-darkness and the added obscurity of the low *iglus* make necessary, profitable and pleasurable the turning of night into day.

Among French folk beliefs as to the night-comings of the devil are these:

1. If a chair or a stool is left upside down the devil will come.
2. If any one goes to bed without moving the chair on which he sat when taking off his shoes, he will be visited by the devil.
3. If a woman looks at herself in a mirror after sun-down, the devil will appear.
4. If dances at farms and in inns do not stop at midnight, the devil will come and take part.
5. The devil will appear to those who talk too much about him after sundown.
6. Whistling or singing outside the house at night attracts the devil.
7. If on hearing a noise at night one turns his head, the devil seizes the occasion to injure him.
8. To make the sign of the cross after sunset will bring on the devil.
9. To work in the fields at dark of night will cause the devil to appear and carry off the laborer if he persists in his toil.
10. If a woman with child goes out after twilight, the devil will take possession of her offspring.
11. Women just after child-birth and children before baptism must not be out at night because the devil roams about from one *Angelus* to the other.
12. After sunset, no woman must go abroad carrying in her arms a child of less than a year or one not yet weaned, else the devil will twist its neck, flatten its head, or carry it off altogether.
13. When women give birth to children at night, the chorus of devils sings above the houses.

Many of these things apply as well to evil spirits other than the devil, to the spirits of the dead, to goblins and monsters of all sorts, to witches, sorcerers and other malevolent beings, who are most active and most dangerous to mankind during the hours from dark to dawn. Curiously enough, the Chinese, whose ideas are so often quite the reverse of ours, believe that "the gods walk abroad by night," and with them night, as the poet Young has termed it, is, "the felt presence of the Deity." But, even with us, the power of evil is sometimes shorn of its nocturnal strength.

In the play of *Hamlet* (Act. 1, Sc. ii), after the exit of the Ghost, the following conversation occurs:

Ber. It was about to speak when the cock crew.
Hor. And then it started like a guilty thing
Upon a fearful summons. I have heard

The cock, that is the trumpet to the morn,
Doth with his lofty and shrill-sounding throat
Awake the god of day, and at his warning
Whether in sea or fire, or earth or air,
The extravagant and erring spirit hies
To his confine: and of the truth herein
This present object made probation.

Mar. It faded on the crowing of the cock.
Some say that ever 'gainst that reason comes
Wherein our Saviour's birth is celebrated,
The bird of dawning singeth all night long.
And then, they say, no spirit dare stir abroad,
The nights are wholesome, then no planets strike,
No fairy takes nor witch hath power to charm,
So hallow'd and so gracious is the time.

Hor. So have I heard, and do in part believe it.

Night, in the old heathen days, was the time for the deeds of "extravagant and erring spirits," of sea, fire, earth and air, of fairies "take" and witches' "charm," and it is an evidence of the impression made upon the folk-heart of Europe by the story of Bethlehem that at the "hallowed and gracious" time of the birth of Jesus all these evil powers had their activities temporarily annulled. The advent of dawn had rendered them powerless before, but now the coming of the infant Saviour stemmed the tide of their life at its most approved epoch. Surely the "Holy Night" was well thought of among men. The folk-lore of the "cock-crow" and of the "Holy Night" deserves special treatment and are too voluminous to be considered here. A whole book might perhaps be written on the single topic of the Child Jesus and the night.

5. *Night as Bogy*. In Upper Brittany night appears as a bogy to scare children with: "Go home! Night will run off with you! Night's coming after you." At Matignon (Côtes-du-Nord) night appears as a great black woman, who lives in the west, between earth and sky, and frightens children who do not get home at the proper time. In Morbihan, when children do not want to go to bed, they are told that "Madame la Nuit" will come to take them. She is represented as a big black woman; and, when children will not let themselves be cleaned up, they are told that their face will be as black as that of Night. At Saint Brieue there was a "cave of Madame la Nuit," into which little boys furtively threw stones and then ran away. But, usually, mankind has relied upon darkness in general, in thought personifying it, as the "bug" with which to "fear boys." Sometimes, however, the night-bogy is a witch in animal shape (owl, vampire, dragon, wolf, etc.), or

some horrible ogre or misshapen being of human kind. Night, as bogy, however, is not always conceived of as a woman. In Scottish folk-lore we find "Auld Daddy Darkness," who is "black as a blackamoor, blin' as a mole."

6. *Night and Marriage*. The consummation of marriage in the hours of darkness is a practically universal custom among the races of man, though there are still some exceptions and there have been, doubtless, more in times past. In spite of the proverb "happy is the bride on whom the sun shines," and the efforts of ecclesiastical authorities at various periods to confine the marriage ceremony to morning, high noon, etc. (in the sixteenth century we find unscrupulous priests rebuked for uniting couples in "the night season," or "by the light of the moon"), a large proportion of weddings take place in the evening, the hospitality of that portion of the twenty-four hours adding naturally to the festivities and rounding out the ceremonies in various ways. The Church of England law (4 Geo. IV, c. 76) required that all marriages must be celebrated between the hours of eight and twelve in the morning," but this was latterly changed (49 and 50 Vict., c. 14) to between the hours of eight in the forenoon and three in the afternoon. The fashion of "wedding-journeys" also favored day-marriages, until the appearance of sleeping-cars and steamboats with "staterooms" tended to turn the tide somewhat the other way. Among many more or less primitive peoples (Santhals, Maoris, Babar Islanders, etc.), and some partly civilized, the marriage ceremony takes place in the evening or at night. With the ancient Romans the bridegroom had to go to his bride in the dark, and in Sparta he had to leave her before daybreak, so that "sometimes children were born before the pair had ever seen each other's faces by day." This surreptitious exercise of conjugal duties by the bridegroom at night is found here and there all over the globe, where darkness is thought to be the proper veil for these events, and where men seek to avoid the dangers lurking in the "contagion of sight." Sometimes both bride and groom are secluded for days, *e. g.*, among the Minahassas of Celebes, they are shut up in a dark room for three days and nights, and elsewhere often for a much longer period. The taboo against seeing the fairy bride "by candle-light" appears in certain folk-tales.

Crawley, who has paid much attention to the study of human taboos, prohibitions and devices for the avoidance of danger, particularly in the congress of the sexes, remarks: "Weddings very commonly take place in the evening, or at night, a custom natural enough for its convenience and its obviation of dangers, such as that of the evil eye and those connected with human, and especially with female, shyness and timidity." To this

he adds significantly: "Taken in connection with the last custom [greeting of the rising sun by the young pair], we may, without excess of fancifulness, note the coincidence with nature's method of shrouding her processes of production in mystery and darkness, and of revealing their results in the light." Behind the bridal veil and other means of concealing the woman in part or altogether from her husband, etc., lies a wealth of folk-lore. Hiding the bride, or keeping her in a dark corner, figures in many wedding ceremonies, and when real darkness is not present, fictitious night is called into play.

When the "daughters of the gods" have to do with the "sons of men," or these with the former, it is often only by night that they can meet each other. It was thus in the Maori legend of the hero Tawhaki and the celestial maiden Tango-tango, who fell in love with him, and nightly came to him, stealing home with the advent of dawn. There are many variants of this tale among the Maoris and elsewhere in the world of savagery and barbarism, not to speak of more civilized peoples. The "heavenly bride," "the ghost wife," "the fairy mistress" are all often but nocturnal visitants, compelled to return to their superhuman dwellings at the first peep of day or at the stroke of midnight. Often, too, the child of "the walker by moonlight" has to be suckled or saved from some great danger by its "uncanny" mother, whose power to aid it lies wholly within the hours of darkness, or is even confined to midnight alone,—at which time only, *e. g.*, "the lady of the lake," in response to the child's cry and the nurse's appeal (as in a Kaffir tale) rises out of the water and puts it to her breast.

7. "*Fair by night and foul by day.*" One of the most noteworthy motives in popular legend and fairy-tale is that of the wife or husband who is "fair by night and foul by day,"—a beautiful human being during the hours of darkness, but a serpent, a dragon, or some other ugly or loathsome creature, if not a hideous man or woman, after dawn. The typical case in literature is "the foul old wife," who figures in Chaucer's "The Wife of Bath's Tale." This famous story and its numerous analogues in other languages than English have recently been studied by Prof. G. H. Maynadier. In Chaucer the knight, who has married the old wife ("a fouler wight there may no man devise"), is told: "Choose, now, to have me foul and old and a humble wife till I die, or young and fair, and take your chances of my lovers." When he told her to please herself, the old hag became "as true a wife as ever was, and fair as any lady or queen in the whole land," and they lived happily ever afterwards. In the ballad of *The Marriage of Sir Gawaine* (ante Charles I), "the lady gives Gawaine his choice

to have her fair by night and foul by day, or *vice versa*." The knight leaves the matter to her and she then declares that she will be fair always,—she had really been bewitched by her stepmother. In the Saga of Hrólf Kraki (14th century) it is told of King Helgí that one night "a wretched female shape" sought entrance to his hut and bed. The king admitted her, but lay all night with face averted from her ugliness. But, "when at daybreak he looked round, a creature of surpassing beauty lay by him, who at once began to fill him with love." This boon of a prince's bed was the ending of her enchantment, for she, too, had been bewitched by a cruel step-parent. In many tales and legends in which the "fair-foul" motif is employed the maiden or youth is awakened from an enchanted sleep or restored to a fair human from a foul bestial shape by the kiss of a lover hero or heroine. Often the "loathly lady" was a good fairy, as Maynadier suggests, who was not under spells at all, but was merely wishing to test her favorite hero. To such a motif various clumsy additions may from time to time have been made. Among the changes of form from night to day cited by Maynadier from the folk-literature of all lands are the following:

1. A prince by night, gray bear by day (Celtic).
2. Gray wolf by day, gentleman by night.
3. Maiden by day, serpent by night,—recovers for good her human form after a third kiss (Rhaeto-Romanian).
4. Prince by night (*i. e.* after sunset), frog by day (Breton).
5. Boy by day, werwolf by night (Breton).
6. Man by day, bear by night (Scandinavian).
7. Man by night, white bear by day (Scandinavian).
8. Prince by night, lion by day (German).
9. Prince by night, crab by day. Also alternately man and eagle (Greek).
10. Young man by night, pumpkin by day (Wallachian).
11. Prince crane by day, man by night (Scandinavian).
12. Man by day, worm by night (Scandinavian).
13. Man by night and hoodie by day, or *vice versa* (Scotch Highland).
14. Man by night and white dog by day, or *vice versa* (Irish).
15. Man by day, seal by night (Irish).
16. Man by night, ram, salmon, eagle by day (Irish).
17. Princess by night, swan by day (Irish).
18. Man by night, raven by day (Gascon).
19. Prince with monkey's head by day, handsome youth at night (Norman).
20. Princes swans except for a quarter of an hour every evening (German).
21. Women by night, stags by day (German).
22. Handsome youth by night, lizard by day.
23. Tree by day, beautiful youth by night (S. Amer. Ind.).
24. Princess a raven except for one hour each night (Armenian).
25. Man by night, snake by day (Servian).
26. Man by night, bird by day (Scotch).
27. Man by night, ring by day (German and Servian).

A story of the rattlesnake (by day) youth (by night) kind has recently been reported from the Tunika Indians by Swanton, and the myth is "of a common American type."

Sometimes, it will be readily seen, the human being appears in proper form by day, sometimes by night. How much of this motif of the alternation of the good and the bad, the beautiful and the ugly, the human and the infra-human (or ultra-human), is due to the contemplation of the natural alternation of day and night is difficult to determine. Doubtless, the metamorphosis of day and night had not a little to do with suggesting the changes of man into beast and *vice versa*.

8. "*Nightmare*." The "nightmare has been a very fertile source of mythology and folk-lore. The "mare" in this word has, of course, nothing to do with *mare* (or *horse*), but is by folk-etymology so interpreted,—in Icelandic, *e. g.*, the cognate *mara* means an ogress. The Latin term for nightmare, *incubus*, from *incubare*, "to lie down upon," connotes the oppressive feeling about the chest, which is the starting point for the nightmare psychosis. With the modern peoples of Europe alone, leaving out of count altogether the uncivilized races of the globe, the lore of the "nightmare" and the "incubus" is very extensive. It was Laistner who first emphasized the *rôle* of the nightmare and the incubus, with dreams, in the production of mythologic phenomena and he was followed in this direction by Mogk, Meyer, Golther, etc.

Among the "swan-maiden" myths Hartland notes a "nightmare type." In the belief of many people in peasant Europe (Germany and the Slavonic countries), "the nightmare is a human being—frequently one whose love has been slighted, and who, in this shape, is enabled to approach the beloved object. It slips through the keyhole, or any other hole in a building, and presses its victim sometimes to death. But it can be caught by quickly stopping the hole through which it has entered." In folk-tales a man stops up the means of egress and suddenly finds in the room "a young, lovely maiden," whom he marries and by whom he has children. If questioned about her nightmare origin, or for other reasons, she sometimes vanishes after years of happy wedded life. Sometimes the nightmare, when caught in this way, turns out to be a beautiful child, who marries and, in like manner, disappears when the question of her origin is discussed. Often the nightmare slips out and in again if an opening is accidentally left. There are many curious folk-beliefs concerning these "nightmares." In North Germany, *e.g.*, "when seven boys, or seven girls, are born in succession, one of them is a nightmare." From the distressful *incubus* to the elusive beauty is a long step, but the folk-mind easily makes it.

9. *Night and work.* In a certain sense, night was the first *schola* or leisure of man, and long continued to be such. It was only with the advent of fire that night was permanently and successfully "tamed," industrialized and estheticized. The discovery of fire really genialized night for man, making possible many of the social arts, including letters, song, and music, to a large extent, at least. Even-songs, "night-thoughts," and "musings, while the fire burned," have added much to the stock of human knowledge.

Says Groos, with some exaggeration, perhaps: "Of children and young animals it is true that, except when they are eating, they play all day, till at night, tired out with play, they sink to sleep." This is before the school interferes with the natural activities of childhood, or social necessity makes them all too soon feel the burdens of labor. For the child, day-time is play-time, and night-time is rest, and both are, or should be, equally genial. Civilization, however, has long ruthlessly disturbed both by the evils of child-labor in the factory and the mine, from which, even now, children of both sexes still suffer much. The night-labor of children has not yet completely disappeared from the Old World or even the New. Those to whom night belongs of right have not all received it.

Woman is perhaps more night-minded than man. In another place the present writer has observed: "Traces of the night-inspiration, of the influence of the primitive fire group, abound in woman. Indeed, it may be said (the life of southern Europe and of American society to-day illustrates the point abundantly) that she is, in a sense, a 'night-being,' for the activity, physical and mental, of modern women (revealed *e. g.*, in the dance and the nocturnal intellectualities of society) in this direction is remarkable. Perhaps we may style a good deal of her ordinary day labor 'rest,' or the commonplaces and banalities of her existence, her evening and night life being the true genius-side of her activities." And "in acting and dancing [and we may perhaps add singing], two professions especially of the night, woman shows marked genius, exceeding even that of man." Recent years, however, have seen an extensive utilization of the afternoon for the meetings of woman's clubs, which now compete or coalesce with pink teas and like post-meridian functions.

The Masai of eastern Africa believe that the night is a man, whose wife is the day. They explain this by saying that "men, who are strong, go and fight the enemy at night, whilst women, who are weak, can only work by day." The amount of work done by women among many primitive, and even civilized, peoples, is so great that not a few writers would agree with the exaggerated statement of Gen. Dodge, cited by Prof. O. T.

Mason, that "it is a god-send that the Indian woman did not know how to make a light sufficient to work by at night." Then, surely, woman's work would never be "done."

But, although mankind have accomplished so much work at night. and the praise of the pioneer student who picked up his education by the uncertain light of the camp or cabin fire, is repeated on behalf of him who "burns the midnight oil," or keeps the electric lights going till long past that fateful hour, there is some evidence that the conquest of night achieved by civilization has not been equally powerful everywhere. The familiar gospel hymn with its refrain,

> "Work, for the night is coming,
> When man's work is o'er,"

goes back to the saying in the New Testament (John ix, 4), attributed to Jesus: "I must work the works of him that sent me, while it is day: the night cometh when no man can work." But, if no man can work in the night, the folk-lore of all lands represents Satan as losing no hour of darkness in his nefarious labors. Still the poets have emphasized the restful night, as *e.g.*, Montgomery, who begins his poem on night thus:

> Night is the time for rest;
> How sweet, when labors close,
> To gather round an aching breast
> The curtain of repose.

Before him, however, Watts had warned that "Satan finds some mischief still, for idle hands to do."

In certain parts of France the folk-belief obtains that working in the fields after nightfall is dangerous. If one does so, he risks coming into contact with headless men, sprites and demons of all sorts. In upper Brittany, if a man works in the fields at dark of night, the devil comes beside him and performs the same actions as he, and, if the laborer does not cease his work, carries him off. Sometimes, the spirits warn men who trespass upon the hours of night that are properly theirs. A legend from Ille-et-Vilaine states that a peasant, who was cutting grain at night, heard a voice call out to him twice: "You must leave the night to those to whom it belongs!" Another from the Ardennes reports that a giant said to those whom he found working at night in the forest: "The day is for you, the night is for me."

Recent studies of work and fatigue by the physiologists and psychologists have revealed the presence of at least two types of "workers" among scholars and men of various occupations and professions, the "night type" and the "day type," perhaps also a "morning type," and an "afternoon type." There are also not a few cases of individuals who get up at night to work,—

sometimes being at their best at two or three o'clock in the morning.

10. *Night and meals*. Even among civilized peoples, meal-times are by no means confined to the hours of daylight. Dinner, the chief meal, may take place at the noon hour or shortly after, at about six o'clock, or often much later in the evening. For social functions and kindred affairs eight o'clock is a common hour for dining. Supper, in England and France in the Middle Ages, and for three centuries after, was generally at about four or five in the afternoon. With regard to the Parisians, Mallery notes: "A substantive change even with them is the hour of the later meal (*i. e.* dinner), which is late in the evening, or in the night, instead of early in the afternoon, as it was a few generations ago." Here, as elsewhere, a shift from day to night has taken place. In our northern climate "lunch" at noon and "dinner" so late that artificial light has to be resorted to are quite fashionable, the night dinner in winter time being more characteristic by reason of the earlier oncoming of darkness. Add to this social affairs, church and theatre suppers, the "night lunch," etc., and it will be seen at once how large an amount of food civilized man is in the habit of consuming after daylight has practically disappeared. To the lamp have now succeeded gas and the electric light, making night activity in the matter of eating sociable and pleasurable to a degree. With primitive peoples the principal meal occurred very often (*e. g.* the Hupa Indians of California) in the evening, sometimes towards noon-day. But there were often other minor meals.

11. *Night and hospitality*. The necessity of asking for "a night's lodging" is one of the turning points in many a folk-tale and fairy story. That night should be the time for the exercise of hospitality is natural enough, since even now most of the moving about and travelling in the world are done by day. The ancient Hindus believed that "he who entertains guests for one night obtains earthly happiness; a second night gains the middle air; a third, heavenly bliss; a fourth, the world of unsurpassable bliss; many nights procure endless worlds." The duty and the exercise of hospitality were all the more necessary and serviceable since night was often the time when not alone the spirits of evil, but the souls of the departed and even the gods themselves walked the earth. If hospitality during the hours of daylight were commendable, it was doubly so after nightfall. At night, especially, did men feel the injunction of the primitive Ainu, "Do not treat strangers slightingly, for you never know whom you are entertaining," a thought which, in higher form, has found a place in our Bible, where we read (Hebrews xiii, 2): "Be not for-

getful to entertain strangers; for thereby some have entertained angels unawares." Night always has been, indeed, the time of "entertaining angels unawares." It is easier for all creatures to show their real colors by day. This very fact has tended to deepen the feeling of hospitality everywhere among men in all ages of the world's history. To keep any one "over night" is still the common expression of the art of hospitality. And often, since at night the whole family is present, the women in particular, hospitality has not infrequently gone to such extremes among uncivilized peoples in various parts of the globe that the *jus hospitis* has included the provision of a temporary consort or the assumption of conjugal rights over some female member of the household.

12. *Night as soother and counsellor.* In the New Testament (Ephes. iv, 26) we find this admonition: "Be ye angry and sin not: let not the sun go down upon your wrath.". And nowadays people are sometimes told, when in a fit of anger, to "go out into the night and cool off." In poetic and in prose literature, as well as in folk-lore, the soothing effect of the "calm and silent" "peaceful," "quiet," "still," "tranquil," "solemn" night is emphasized. Night is, with sleep, the great cure for human ills; anger, worry, despair flee at her coming. As Ovid greatly phrases it: *Curarum maxima nutrix Nox.* Night hides defects that in daylight agitate the minds and souls of men. The French proverb has it, "by night all cats are gray," and the Greek, "when every candle is removed, every woman is alike."

In how many ways is not mankind, to use the Vergilian phrase, retouched by Conington, "released from trouble by the touch of night!" The French proverb, "Night brings counsel:" finds echoes in every land and in every age, from the German "counsel overnight is better," to the saying attributed to the Persians, "counsel must be taken twice; once by day, again at night."

Night and sorrow are easily and commonly associated. Said the English poet James Montgomery (1771-1854):

"Night is the time to weep,
To wet with unseen tears
Those graves of memory where sleep
The joys of other years."

and, both in literature and in folk-lore, we meet with this selection of night as the appropriate period for grief and its expression. The use of fire and its association with religious ceremonies doubtless made funerals and funeral rites during the hours of darkness possible to uncivilized man, and so, much of the ritual of death and of mourning was transferred to the night, when by the light of his own camp-fire, he started off

the dead on their long journey to the world beyond, whose lights he saw burning in the skies above him. And it is no wonder that the Australian aborigines, who conceive of the milky way as the path of souls, see in its stars the camp-fires kept going by faithful wives, who have preceded their husbands, to light them on their journey to the spirit-land. Through darkness into light passes the soul of man at death, as at birth he came forth out of darkness into light.

13. *Weather-lore of night.* In literature and the sayings of the folk are to be found not a few prognostications of the weather of the morrow from the aspect of the night. In one of his sonnets, Shakespeare develops a fine figure of speech from this source:

> "Do not drop in for an after-loss.
> Ah, do not, when my heart hath 'scap'd this sorrow,
> Come in the rearward of a conquer'd woe;
> *Give not a windy night a rainy morrow,*
> To linger out a purpos'd overthrow."

One of the weather sayings of the African Masai is this: If, when the sun rises, the heavens are red, it will rain; if, when the sun sets the sky is the color of blood, some warriers out riding have been successful. It is not a far cry from this to one of the most famous of weather-proverbs, thus stated in the New Testament (Matthew xvi, 2, 3): "When it is evening, ye say it will be fair weather, for the sky is red; and in the morning it will be foul weather to-day, for the sky is red and lowering."

This has its counterpart in the common English sayings:

> Evening red and morning gray
> Will set the traveller on his way;
> But evening gray and morning red
> Will bring down rain upon his head.
>
> If the sun in red should set,
> The next day surely will be wet;
> If the sun should set in gray,
> The next will be a rainy day.
>
> An evening red and a morning gray,
> Are sure signs of a fine day.
>
> Evening red and morning gray
> Two sure signs of one fine day.
>
> An evening gray and a morning red
> Will send the shepherd wet to bed.
>
> Red skies in the evening precede fine to-morrows.

And more to the same effect.

The "rainbow at night" is noted in our English folk-lore:

> Rainbow in the morning,
> Shepherds take warning;

Rainbow at night,
Shepherd's delight.

A rainbow in the morning
Is the shepherd's warning;
A rainbow in the night
Is the shepherd's delight.

Rainbow at night,
Sailors' delight;
Rainbow in morning,
Sailors' warning.

If there be a rainbow in the eve,
It will rain and leave;
But if there be a rainbow on the morrow,
It will neither lend nor borrow.

The rainbow proverb is also expressed in more scientific language as follows: "Rainbow in morning shows that shower is west of us and that we shall probably get it. Rainbow in the evening shows that shower is east of us and is passing off."

Some proverbs and sayings relating to the actions of animals at night as weather signs are:

1. Bats flying late in the evening indicate fair weather. Bats who speak flying tell of rain to-morrow.
2. When swallows in evenings fly high and chirp, fair weather follows; when low, rain follows.
3. If the cock goes crowing to bed,
He 'll certainly rise with a watery head.
4. When grouse drum at night, Indians predict a deep fall of snow.

14. *Night as a measurer of time.* Among civilized peoples at the present time, as our common proverbs and sayings indicate (*e. g.* "every dog has his day"), measurement by day is so predominant that we have almost forgotten that our ancestors reckoned by night, that there was ever a time when not "in the days of——," but rather "in the nights of——" was the proper phrase; or, again, "Give us our *nightly*," not our "*daily* bread." As the moon was the measurer of longer periods of time, so the night was of shorter ones. The emphasis placed upon "night" is revealed by the fact that while, in the terms for "day" significant differences occur among the various Aryan languages, there is practical agreement in the words for *night* (a common Aryan stem *nokt* is recognized by philologists). This leads Kluge and others to argue that the primitive Aryans counted by "nights" and not by "days." Relics of such a procedure are to be found in our own English "sennight" and "fortnight," and in the German terms *Weihnachten* (lit. "holy night") and *Fastnacht*, etc. In the Rig-Veda occur such passages as this, which Schrader cites: "Let us celebrate the old nights (day) and the harvests (years)," and the Sanskrit *nicanicam* (literally "night for night") means "daily." Schrader thinks that in the oldest times the moon was the

measurer of time and day-reckonings only came into vogue with greater appreciation of the sun and the rise of a solar cult. Then, as the term *night* formerly included *day*, so the term *day* came later to include *night*.

The use of *eve* in English, *Abend* in German, in such ceremonial and holiday terms as New Year's Eve, Christmas Eve, Hallow e'en, St. Agnes Eve, Candelmas Eve, Easter Eve, St. Mark's Eve, etc., Christabend, Sonnabend, etc., to designate the "night before" a holiday or cult-period and sometimes even the day itself is worth noting.—*Sonnabend* means now all Saturday and not merely the part of it which just precedes Sunday; and *Weihnachten* includes likewise all of Christmas. And with the anniversaries of certain saints the "day" and "eve" are completely fused. In heathen times, before the word was consecrated to Christian uses, *Weihnachten* meant in German the "day before" the great winter festival celebrated from the 26th of December to the 6th of January, the "Yule" of older English. In reality, the old Germanic reckoning of time counted evening and night with the following day,—the diurnal period was from eve to eve, night being thus characteristically emphasized. From sunset to sunset has been a common method of computing the day of 24 hours, particularly for ecclesiastic purposes. The Hebrew Sabbath is of this nature, as was the "day" of the ancient Greeks. And in the story of creation in our Bible we read (Gen. i, 5): "And the evening and the morning were the first day." In older speech, as the German *Morgen* still indicates *morning* meant the early *morrow*. Both *night* and *morning* have thus at times signified the whole day, as *eve* and *morrow* have also been included in one complex. To count the "day" from sunset to sunset appears to have been a very common practice among the races of man (Hebrews, Greeks, Gauls, Teutons, etc.), and some have sought to bring it into relation with the moon-cult. The limits of "day" and "night" are often rather indefinite and peoples differ not a little as to their distinctions. In the Talmud-Mishna lore of the Hebrews this definition is found: "When the eastern sky becomes pale, it is twilight; when the eastern sky becomes so black that the upper and lower parts of the sky are of the same color, it is night." The "watches of the night," too, vary with different peoples. The Masai idea of night is thus reported by Hollis:

> The evening is the time when the cattle return to the kraals just before the sun sets (6 P. M.).
>
> There is also the time called nightfall, or the hour for gossip (8 P. M.); this is the hour before people go to bed.
>
> Then there is the night, midnight, and the time when the buffaloes go to drink—this latter is the hour before the sun rises, which the Swahili call *sava kumi* (4 A. M.).

There is also the time called the blood-red period, or when the sun decorates the sky; this is the hour when the first rays of the sun redden the heavens (6 A. M.).

Then there is the morning; this is after the sun has risen.

For the devil, who needs no sleep, the twenty-four hours, in folk-belief, often constitute two days, as his dupes in a compact with a time-limit discover to their sorrow. To the devil "nights are days," runs a French popular saying. A Picardy legend represents the devil as saying, when he came to demand the fulfillment of the pact earlier than his victim expected: "For us the day lasts from six in the morning to six in the evening; and from six in the evening to six in the morning is another day."

15. *Night vs. Day*. The opposition of day and night is a common feature of primitive mythologies. In Polynesia this opposition was highly systematized. On one side are the "dwellers in day," at the head of whom is Tangaloa, fair-haired and light-skinned, the beneficent god of daylight, inventor and promoter of the arts and activities of peace; on the other, Rongo, dark-haired and dark-skinned, the god of night and gloom, the stirrer-up and author of strife, bloodshed, war, and every form of discord and dissension. To one or other of these two classes belong all the gods. In several modern religions this same antagonism is reflected in the contest between the "God of Light" and the "Prince of Darkness,"—Christianity to-day has much of this belief still left in it. In the Middle Ages the idea was strongly entertained that Satan had vast power during the night, "until cock-crow," when God and his good angels regained their complete ascendency.

In many myths, particularly in some from the Indians of the northwest coast of North America, the occurrence of day is made possible by opening the box or other receptacle in which the light is confined, and letting it out. This feat, performed by cunning and stealth, is often one of the great achievements of the tribal heroes and demi-gods, *e. g.*, the raven, who figures prominently in this regard. Like the stealing of fire, the theft of light is a prime *motif* in every quarter of the globe. Sometimes sun, moon, stars, and light in general are separately or successively obtained for man from witches, demons, deities. And the myth of their origin is often very complicated. In some legends, the oncoming of day has a remarkable effect on the men of the primitive world. Thus, in a legend of the Tlingit Indians we read:

"When it became day and the men on earth saw each other, they all ran away from one another. Some turned into fish, others into bears and wolves, others again into birds. And in this manner arose all the various kinds of animals."

One would have expected such a result from the confusion of night rather than from the light of day. It suggests comparison with the actions of Adam and Eve after their obtaining the fruit of the tree of knowledge. But the "daze" of light is a common human experience, not unnoticed by the most primitive tribes of mankind.

Over against the miracle attributed in the Old Testament (Jos. x, 12-14), when "the sun stood still in the midst of heaven, and hasted not to go down about a whole day," and "the sun stood still, and the moon stayed, until the people had avenged themselves upon their enemies," may be placed the wonders reported at the funeral of St. Patrick, when, "for twelve days there was no night, no twilight, no darkness at all through the whole province; nay, for a whole year the nights were more luminous than usual and the clouds less heavy."

According to a myth of the Wiimbaio tribe of Australian aborigines, the sun never set at all in the beginning and it was light all the time. The people did not like this and so Nurali, a supernatural being, sang a song to this effect:

"Sun, sun, burn your wood, burn your interior substance, and go down!"

And so now the sun, after burning up his fuel during daytime, has to go far below the horizon to get more. Thus we have the alternation of day and night. Another Australian legend states that at night the sun, who is a woman, plunges into the earth, or into the water, to get lizards, roots, or fish, for she is hungry. In yet another, the sun is a traveller, who comes and goes. Other similar legends are found scattered all over the globe, among peoples of human races.

16. *Alternation of night and day*. The present alternation of night and day is not, according to many savage and barbarous peoples, the original state of affairs. This is evident from a glance at numerous creation myths. In our Bible we find the creation story of the Hebrews, wherein it is stated:

"In the beginning God created the heaven and the earth. And the earth was without form and void; and darkness was upon the face of the deep. And the spirit of God moved upon the face of the waters. And God said, let there be light; and there was light. And God saw the light, that it was good; and God divided the light from the darkness. And God called the light day, and the darkness he called night. And the evening and the morning were the first day."

Here we have the creation of light out of darkness by divine fiat, a procedure met with in ruder form in the creation legends of peoples all over the globe. This process is, however, entirely different from that appearing in many mythologies, where the question to be settled is often whether there shall be

eternal darkness or unending day. Sometimes one set of gods desires the former and another the latter, and a compromise has to be effected, resulting in the present alternation of daylight and dark.

That the alternation of night and day should be connected with gambling is natural enough. In the great gambling myth of the Navaho Indians, the action begins with an original alternation of day and night as we now have it, which was not satisfactory to all creatures dwelling upon earth:

"In the ancient days, there were, as there are now, some animals who saw better, and were altogether happier in the darkness than in the light; and there were others who liked not the darkness and were happy only in the light of day. The animals of the night wished it would remain dark forever, and the animals of the day wished that the sun would shine forever. At last they met in council in the twilight to talk the matter over, and the council resolved that they should play a game of hiding a stone in a moccasin (as in the game now called *Kesitce*) to settle their differences. If the night animals won, the sun should never rise again; if the day animals succeeded, never more should it set. So, when night fell, they lit a fire and commenced the game."

In this game some of the gods joined with the animals of one side or the other, whispering secrets, giving advice, etc. The night animals were helped by "the great destroyer," Yeitso, the day animals by the wind-gods. The coyote was too cunning to declare absolutely for either side, and so "he usually stood between the contending parties, but occasionally went over to one side or the other as the tide of fortune seemed to turn." The game was long and the streaks of dawn appeared on the horizon before it could be finished. The animals broke up the play in confusion, each hastening home as fast as he could. And, the tale concludes: "As the animals never met again to play for the same stakes, the original alternation of day and night has never been changed."

17. *Night and literature, art, etc.* It is not by accident that the best known collection of stories from the Orient bears the title of the "Arabian Nights." This reminds us of the fact that night (or evening) is the time *par excellence* for tale-telling all over the globe,—the camp-fire and the story still belong together, as they have done from the beginnings of human history. In the cold north the long winter evenings, and in the warm south the beautiful starry skies of night, created and sustained literature. The suggestive power of night has been infinite,—darkness, moon and stars, quiet, noise, etc., all have had here their influence. Night is poetic and mythopoeic. The poet has well said:

"Night hath made many bards; she is so lovely.
For it is beauty maketh poesie,
As from the dancing eye come tears of light.
Night hath made many bards; she is so lovely.
And they have praised her to her starry face
So long that she has blushed and left them, often."

But the ugliness of night, no less than its beauty, has been stimulating. This appears from hundreds of myths and legends in every part of the habitable earth that tell of what has been

"Swallowed up and lost
In the wide womb of uncreated night."

But the "lone watcher of the skies" has perhaps given mankind more of good than of evil, more of pleasure than of pain, from his inspirations. And it was to "the shepherds watching their flocks by night" that the angels on high sang the song to whose music the whole world shall some day move. Man, stimulated to song by the aspect of the nightly heavens, attributes to the cosmos his own feelings and emotions, and hears "the music of the spheres," begun when "the morning stars sang together, and all the sons of God shouted for joy." It is not surprising, therefore, that among some more or less primitive peoples tale-telling in the daytime should be tabooed, and music and the dance confined almost to night alone. Fire stimulated to an unwonted degree the activities of man at night and "tamed" the hours of darkness for the service of man, a service which the discovery and extended employment of the torch, gas, and electricity have now made practically universal.

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SOME ASPECTS OF MEMORY IN THE INSANE [1]

By Florence Berenice Barnes

Report of Experiments Performed in the Psychopathic Ward of the University of Michigan

The object of the experiments was to repeat on the insane certain of the experiments that have been used with normal individuals in the endeavor to understand in more detail the changes of memory in insanity. Our particular problem was to determine the rate of learning and degree of retention as compared with the normal and to study the relative value of 'heaped up' vs. divided repetitions and the influence of generative, effectual and retroactive (Rückwirkende) inhibition.

In the experiment the learning method and the Treffer method were used in approximately the pure form.[2] In the learning method, it will be remembered, impression is measured by the number of repetitions needed for the first perfect repetition, and the degree of retention by the number of additional repetitions required after the lapse of a given time to bring the series back to one perfect repetition. In the Treffer method impression is measured in the same way, but retention is tested by the number of cases in which the second syllable in a pair can be supplied when the first is suggested. Ordinarily the syllables are given successively and paired by accenting every other syllable. In addition in our work the syllables were paired by writing two syllables side by side on the cards. In the experiments by both methods both the one column and two column cards were used. The one is referred to in the text as the learning or reading syllables in single order, the other as learning or reading in double order.

It may be interesting to note that it is much more difficult to make out lists of nonsense syllables in English than in German, for the English language has a large number of words of three letters while the German has comparatively few. The number of nonsense syllables left after striking out those that make sense from the combinations of letters is correspondingly small. Our diphthongs, too, are less numerous than the German diphthongs and umlauts, and diphthongs in our experiments were found to confuse the subjects to a very marked degree.

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[1] Communicated by W. B. Pillsbury.
[2] Ebbinghaus: Grundzüge d. Psychologie, 2 Aufl. p. 646 ff.

It was found possible to perform the whole series of experiments with but two subjects, patients suffering from syphilis of the brain with mental disturbances. These patients were subject to convulsions every few weeks and would have no memory after the attack of what had occurred to them during the attack. In fact, they knew that they had been ill only by the feeling of dizziness and weakness, and the general soreness that resulted from the convulsions. Both subjects were easily fatigued, and after a very short time of testing would complain of dizziness and the complete vanishing of the impressions from mind when the card was removed. In addition to these two patients a number of others were used for one or more problems. A number of forms of mental disease were represented in the different experiments. The others could not be counted on regularly, however, as they are often incapable of learning a series completely, no matter how great the number of repetitions, and in the circular cases there would frequently be days when the patient would be incapable of working. The tables will indicate the forms of insanity to which the patients were subject.

Certain general difficulties in the conduct of experiments upon the insane may be noted in advance in connection with the departure from the ordinary course of procedure that they necessitate. The conditions of the subjects vary not merely from day to day, owing to the stage of excitement or depression in which the subject is found, but they vary from moment to moment. Nearly all cases are very much given to introspection. They are constantly thinking of their own condition and one can never say when the learning will be interrupted by some impelling thought of themselves. This may be suggested by the syllables, or by some chance incident, internal or external. Coupled with this attention is easily distracted, and will seldom be given long to one subject or task. It was this that compelled the use of the *Treffer* method. For in the method of learning repeated mind wanderings might prevent the learning entirely, a series might never be repeated a sufficient number of times, without distraction, to permit it to be learned, and so no test would be possible, while it would be possible to get a sufficient number of repetitions to forge some links, and these could be tested at favorable moments. By the *Treffer* method the strength of each link of the chain may be tested, while in the learning method the strength of the chain as a whole alone can be taken into account, and the strength of the chain is the strength of the weakest link.

This consideration, too, dictated making the apparatus as simple as possible, so that all elaborate exposure or recording apparatus was done away with and the syllables printed upon

a card and all exposed at once by passing the card to the patient with the request to commit to memory. This method of course brought in disturbing inhibitions but it obviated the necessity of teaching the patient to shuffle the cards at regular intervals, and insured that none should be omitted, through the patient's clumsiness of manipulation.

The first two part problems that we have dealt with are the influence of the different repetitions, and of the number of syllables in a series upon the formation of association. Ebbinghaus[1] found for normal individuals that the repetitions were all of equal value. In the insane this is not at all the case. The first repetition is of much greater value than any of the others. The next eleven repetitions hardly double the number of right responses by the Treffer method. The explanation is to be found in fatigue and wandering attention. In this connection it may be remarked that in the insane much more even than in the normal, failure to learn is due to failure to attend. The number of repetitions is much less important than the character of the repetitions. Attention affects both the strength of the associations and the strength of the perseverance tendency, and upon these two factors alone depends the liability to recall.

As far as fatigue does not enter, we might suppose that the same number of repetitions of a series of syllables would bring the same strength of syllable associations, no matter how long the series is. But many accidental influences may enter to add to or detract from certain associations, and the longer the series is, the more chance they have to act. The same number of readings of a series fatigues according to the length of the series. This results in paying less attention to the last readings than to the first. The longer the series, the more numerous the syllables in readiness to rise to consciousness, and so the number of inhibitions proceeding from them is greater, and a greater number of readings is necessary to learn them.

The first and the last syllables of a series make a stronger impression than those intervening, though sometimes the first syllable, instead of calling more forcible attention to itself simply draws forth attention which is directed on to the second and following. The degree of stress also strengthens the syllable by setting up certain kinæsthetic associations of the tongue and palate.

In Table 1 is given the number of repetitions necessary to learn series containing different numbers of syllables from six to twelve. It will be recalled that with normal individuals six to eight syllables can be learned at one repetition, twelve in fourteen to sixteen repetitions. The disparity against the in-

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[1]Ebbinghaus: Ueber das Gedächtniss, § 19.

sane is increased by the fact that not more than twelve repetitions could be made on the same day without unduly fatiguing the patient, and this must of course be rigorously avoided. In accordance with Jost's law this should greatly reduce the number of repetitions required.[1]

TABLE I

Subjects.	Syllables.	Repetitions.	Subjects.	Syllables.	Repetitions
F—	6	14	S—	6	Infinite.
	8	25	Manic	8	"
Cerebral	10	41	Depressive	10	"
Syphilis	12	49	Excitem't	12	"
B—	6	12	S—Manic	6	"
	8	16	Depressive	8	"
Cerebral	10	37	Depres'ion	10	"
Syphilis	12	48		12	"
R—	6	8	R—	6	3
Paranoid	8	13	Maniacal	8	7
Dementia	10	17	Excitem't	10	11
	12	33	Nearly Recovered	12	14
L—	6	12	S—	6	2
Senile	8	17	Dementia	8	5
Dementia	10	too fatig'ing	Praecox	10	9
	12	" "		12	12

Note that the manic-depressives were unable to make any progress whatever in correctly recalling their series. This was due almost entirely to default of fixation, as before stated. The case of dementia præcox shows little defect of memory.

In the case of brain syphilis, perhaps, a few quotations from the patients' own remarks may serve to clear up these results more than any technical explanation.

"When I try so hard to retain them it increases the pain in my back and it shoots up into my head and makes me dizzy, and when I don't try hard to retain them they all leave me. Sometimes they go so I can't think or get them back at all, and sometimes I imagine I can see them and spell them. When I get lots of words, I get the words on different cards mixed up. Other ideas seldom enter—at least not so much now as they did." [Yet at the slightest interval of rest, patient will begin to talk of her home, her family, her condition, etc.] "The chief interruption has been the pain spells. With the dizziness, the syllables seem to vanish from mind. I think

[1] Jost: Zeitschr. f. Psych. u. Phys. d. Sinnesorg. XIV, p. 436.

of the syllables between times. In the morning when I wake up, I think of the words on the cards and try to repeat them all. I sometimes dream of them. The syllables seem to drop away suddenly and leave an absolute blank."

Inhibitions

There are a number of factors in the nature of the test itself, which hinder or inhibit the rise of the right response to consciousness, or tend to put another in its place. Two kinds of inhibitions noted by Müller and Pilzecker[1] are the generative and the effectual. Syllables already associated with other syllables are with difficulty associated with new ones. This is called generative inhibition. Effectual inhibition is caused by the mutual interference of the tendencies to reproduce each of two syllables which are associated with the one given. These two inhibitions commonly work together and are really two sides of one and the same process.

The test for noting the number and form of these inhibitions was given in the following manner. Two lists of syllables were made out thus:

1)	v_1 u	h_1 u	v_2 u	h_2 u
2)	v_3 u	h_2 u	v_4 u	h_1 u

Let v_1, v_2, v_2, v_4, represent different syllables; h_1, h_2 are respectively the same for each list; u differs in each case.

For instance,

(v_1) poli — (u) sil	(v_3 mag — (u) rit
(h_1) *tim* — (u) laim	(h_2) *lis* — (u) wef
(v_2)cholg — (u) bol	(v_4) paon — (u) loip
(h_2) *lis* — (u) fey	(h_1) *tim* — (u) neap

The two lists were written consecutively on the same card, as given, so that in reading the total number, h_1 and h_2, would be read in connection with two separate syllables each, and the two associations thus set up would be mutually inhibitive.

We will call h_1 and h_2 the chief syllables, since it is most largely with them that we have to do at present, and v_1, v_2, v_3, v_4 the stimuli, and u, in its various forms, the response.

It may be noted that in reading the second part of the list, the chief syllables already met in the first part, have become more or less familiar through this fact, and so tend to draw special attention to themselves, thus lessening the inhibition. Reading the chief syllables of the second part may also recall the syllables associated with them in the first part, and thus strengthen the associations. When both the associations are

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[1] Müller und Pilzecker: Exper. Beitr. zur Lehre vom Gedächtniss. Zeitschr. f. Psych., Ergänzungsband I, pp. 144-168.

given in response to the chief syllable stimulus, we call it a double case. These double cases are of two kinds, according to whether the response of the first part or the response of the second part is given.

The lists are read in the same order at each sitting, but the stimuli are given in a different order each time, that they may be mixed up as much as possible, thus avoiding the elimination of inhibition by rote learning, due for the most part to a sense of the absolute position of the syllable in the series.

A little further explanation of the character of these inhibitions may help to an understanding of the results.

If a syllable h_1 already associated with u, is read in the new combination h_1 u_1 in general a re-arousal of the association h_1 u takes place, in that through the reading of h_1 the syllable u is placed in readiness, or in case the arousal is more intense, even brought into consciousness. This also has the effect, that the association h_1 u, through this re-arousal, gains in strength, or that at least an easier and quicker reproducibility exists for the next time. The intensity of the associative re-arousal depends on the strength which the association h_1 u possesses at the time of giving the combination h_1 u_1. This is dependent on the amount of attention given to the combination h_1 u when it entered consciousness, on the number of repetitions which it received, and on the length of time which separated the reading of the combination h_1 u_1 from that of the combination h_1 u. If by reading the combination h_1 u_1 the earlier formed association h_1 u is aroused, this associative re-arousal has not merely an intensifying effect for the association h_1 u or at least the reproducibility of u, but it acts at the same time as an inhibition for the forming of the association h_1 u_1. This generative inhibition not only takes place when in the reading of the combination h_1 u_1 the syllable u always comes into consciousness after the appearance of h_1 but also when at the appearance of h_1 the syllable u is placed in readiness, and in reality only a recognition of h_1 takes place. The more intense the re-arousal of the combination h_1 u by the reading of the combination h_1 u_1 the stronger is the generative inhibition for the association h_1 u. These inhibitions may not take place in cases of flights of association, since the combination h_1 u may not be strongly enough associated to inhibit the association of h_1 u_1 but in these cases the latter association, weakened by the same cause, is not strong enough to be recalled when the stimulus is given. As a rule, an impulse from a nerve centre along two courses will take the course which has been most frequently travelled, but if obstructed in any way on this course, will turn all the more strongly into the new path. Results of these two forms of inhibition are given in Table II.

TABLE II

Subjects.	Number of Tests.	No. of Inhibitions.	Double Cases.
B—	8 (1st series in dif. orders.)	6	2
Cerebral Syphilis	8 (2nd series in dif. orders.)	4	4
	8 (3rd series in dif. orders.)	5	3
Total	24	15	9
F—	8 (1st series in dif. orders.)	7	1
Cerebral Syphilis	8 (2nd series in dif. orders.)	5	3
	8 (3rd series in dif. orders.)	5	3
Total	24	17	7

In the manic depressives it was impossible to obtain results in these tests. During excitement the number of random associative combinations with syllables of the series being given, or with some other was so great that the strength, or even the existence of inhibitions could in no way be accurately determined, while in depression one cannot tell whether the customary unresponsive attitude of the patient to the stimuli is due to specific inhibition of any particular association or to the general associative retardation peculiar to this state.

These tests were rather too difficult to be applied to the patients afflicted with other forms of insanity mentioned in the results of the simple repetition tests.

From the results given in the tests of the two cases of cerebral syphilis, we see that although a supposed disintegration of the centres has here taken place, thus increasing the difficulty of forming associations, yet when once formed they may have an inhibitive influence on the formation of new ones. It is to be noted, however, that the inhibitions in these cases were for the most part negative rather than positive, that is, it would most often be found impossible to give any response to the stimuli with double combinations, rather than that the syllable combined with one should be given as a response to the other. This shows that the inhibition in these cases is due, not so much to the influence of a definitely formed previous association as to a generally confused and retarded mental state incident to the disease.

Closely related to these inhibitions is the "rückwirkende" or

retroactive inhibition.[1] In the reading of a syllable series certain physiological processes, which serve to strengthen the associations set up by the reading of the series, last a certain time, with gradually decreasing strength. These processes, and their beneficial influences on the associations are more or less weakened, if the person experiences an interval of mental activity immediately after the reading of the series. This mental activity, creating new associations, tends to weaken the old ones,—thus inhibiting their retention and recall,—just as established old ones hinder the formation of the new.

This inhibition is naturally stronger, the greater the attention given to the following mental activity, and the earlier it takes place, so that a period of rest between the readings is advisable if this is to be eliminated. (See discussion of Jost's Law.) Not only mental activity in the form of work results in this inhibiting tendency, but every strong distraction of the attention immediately after the impression of an idea injures its retention. Impressions should have time to *set* or *consolidate*. This is especially important in testing pathological cases, since frequently, though the series is read with seemingly good attention, immediately after reading the subject may be attracted toward or remark about something in the room, or pertaining to his own condition or affairs.

The retroactive inhibition was found to have much greater influence in cases of associative incapacity than in the normal, because the previously formed associations have so little strength that any different activity immediately following their presentation becomes a serious obstacle in the way of their retention. The tests for observing this inhibition particularly were made in three ways: 1, the subject was required to read a series of syllables of the same length as the presentation series, immediately after the reading of the latter. He was then required to recall as many as possible of the presentation series; 2, a piece of poetry or a short newspaper article was read to the subject after he had finished reading the presentation series; 3, the subject was engaged in conversation for a few minutes after reading the series, without his knowing that it was done for a purpose. For instance, his attention would be drawn to something outside the window, or an interesting bit of news would be told him, or rather her, for the subjects were women.

The stimuli consisted of a simple series of eight syllables, and it was found necessary in all cases to have ten repeated readings with two minute rests between, before the interval of different mental activity following, since it will be seen by reference to Table I, that about twenty repetitions are required to learn an

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[1] Müller und Pilzecker: *op. cit.*, pp. 174-198.

eight syllable series perfectly. In the results, the number of false and negative responses divided by two will give approximately the number of cases due to the retroactive inhibition. It was impossible to give twenty readings during a single test, as the element of fatigue, and resultant inattention would have destroyed the value of the results.

TABLE III

1. Reading of dif. series after presentation. (10 series.)

Subject.	No. Syls. Read.	Correct Recall.	False Cases.	Negative Cases.
F—Cerebral Syphilis	80 Av. 8	14 1.4	43 4.3	25 2.5

6.8 ÷ 2 = 3.4 or 42% of responses influenced by inhibition. (10 series.)

Subject.	No. Syls. Read.	Correct Recall.	False Cases.	Negative Cases.
B—Cerebral Syphilis	80 Av. 8	17 1.7	49 4.9	14 1.4

6.3 ÷ 2 = 3.15 or 39.3% of responses influenced by inhibition.

The large number of "false" cases in these results is due for the most part to associative combination of the syllables of the presentation series with those of the inhibitive series. The existence of this combination is none the less an inhibition to correct recall.

TABLE IV

2. Reading matter after presentation. (10 series.)

Subject.	No. Syls. Read.	Correct Recall.	False Cases.	Negative Cases.
F—Cerebral Syphilis	80 Av. 8	20 2	17 1.7	43 4.3

6 ÷ 2 = 3 or 37.5% of responses influenced by inhibition. (10 series.)

Subject.	No. Syls. Read.	Correct Recall.	False Cases.	Negative Cases.
B—Cerebral Syphilis	80 Av. 8	25 2.5	22 2.2	33 3.3

5.5 ÷ 2 = 2.75 or 34.4% of responses influenced by inhibition.

Notice the smaller number of false cases here, as compared with negative ones, as there is little chance for associative combination outside of the presentation series.

TABLE V

3. Conversation after presentation. (10 series.)

Subject.	No. Syls. Read.	Correct Recall.	False Cases.	Negative Cases.
F—Cerebral Syphilis	80	19	29	32
	Av. 8	1.9	2.9	3.2

$6.1 \div 2 = 3.05$ or 38.1% of responses influenced by inhibition.
(10 series.)

Subject	No. Syls. Read.	Correct Recall.	False Cases.	Negative Cases.
B—Cerebral Syphilis	80	27	13	40
	Av. 8	2.7	1.3	4

$5.3 \div 2 = 2.65$ or 33.1% of responses influenced by inhibition.

If the results are closely examined, it will be seen that the inhibitive value is slightly greater than can be accurately determined, for the following reason. We have seen that an average of about twenty repetitions was necessary in these two cases to learn a given eight syllable series correctly. (See Table I.) We have also seen that the value of the individual repetitions decreases with their number. Therefore the first ten repetitions should give more than half correct results, merely because of the number of repetitions, so that the number of false or negative cases due to lack of repetition must be slightly less than half the total number, and those due to inhibition a little more than half, so that, on the whole, we may say that about forty per cent. of the responses in this test were influenced by retroactive inhibition from the various forms of mental activity following the readings of the presentation series.

ASSOCIATIVE COMBINATIONS

Probably the most fertile source of false cases is the tendency to combine elements of one association with those of another. There are two reproduction tendencies, one including the beginning and ending consonants, the other the vowels, and the most frequent combination is giving the consonants of one syllable and the vowels of another, viz.:—zet—kap—kep.

The response may be 1. a combination of the right syllable with the given stimulus; 2. a combination of the right syllable with a syllable following or preceding it in the series; 3. a combination of the right syllable with the syllable occupying the same absolute position in another series; 4. a combination of the right syllable with the syllable with which the stimulus is connected by association with a preceding or following syllable; 5. combination of right syllable with a once given false response to a given stimulus, or a similar syllable; 6. combination of right syllable with similarly stressed syllable in preceding group; 7. combination of right syllable with syllable of related series spelled backward; 8. combination with some word or sound of daily life not entering into the experiment.

To determine the number and character of associative combinations in the pathological cases under investigation, a test was given consisting of ten different series of eight syllables each; five of the series were simple lists, where the subject was required to recall as many as possible; and five were the double lists, where, after reading the series a certain number of times, the subject was required to state the syllable immediately following the chief syllable given as stimulus. With the exception of a case of maniacal excitement, the tendency to combination was greater in case of the former than of the latter, probably because in the latter each association is reinforced in a definite way by giving as stimulus the syllable directly combined with it in the reading. It is obviously extremely hard to classify each combination according to the eight divisions given above. It will suffice to say that one or more were found under each division, —enough to justify the classification which I have given. In the results, only those cases are noted in which the incorrectness was found to be really, or at least very presumably, due to associative combination. Each series was read ten times, both for the double and the single lists.

TABLE VI

Single Lists

Subjects.	No. of Syls.	False Cases.	Assoc. Combinations.
F—Cerebral Syphilis	40	25	19
B— " "	40	20	19
S—Maniacal Excitement	40	36	27

The correct and the negative responses are not listed, as they are of no value in this test. Note the great number of associative combinations in the case of maniacal excitement. These

belonged for the most part under classification 8, being often due to combinations with biblical names and references, having similar sounds. In the other two cases the combinations were for the most part traceable to classifications 3, 4 and 6, showing greater concentration of attention, inasmuch as the associations do not tend to wander outside of the work in hand.

TABLE VII

Double Lists

Subjects.	No. of Syls.	False Cases.	Assoc. Combinations.
F—Cerebral Syphilis	40	22	14
B— " "	40	19	12
S—Maniacal Excitement	40	36	29

In the last case a large percentage of the combinations were due to the sound of the stimulus given,—the tendency was to introduce the vowel of the stimulus between the consonants of the response. The person troubled with flight of association is easily influenced by similarity of sounds of words. This is perhaps seen more plainly in simple association tests, when he is required to tell what he thinks of when certain words are given as stimuli. In a large number of cases he will give a word rhyming with the one given or add a syllable to it to make a longer word. The percentage of false cases which are found to be associative combinations is little greater in case of the abnormal than of the normal. Of course the percentage of false cases is itself much greater than in the normal, so that the tendency to associative combination is also seen to be much greater, but in both normal and abnormal cases, false responses are largely due to this tendency to combine associations.

OTHER CAUSES OF FALSE CASES

A response is often given which is not a combination of the correct response with some other syllable,—but which has no elements of connection with it. This incorrect response may be due to one or more of several reproduction tendencies. The stressed beginning syllable of a group is connected with the unstressed end-syllable of the preceding group by an association of noticeable strength. Carrying this further, we may say that any syllable has a greater or less tendency to recall any preceding syllable of the series, according to the degree of stress of the syllable given, or those preceding. The same rule holds for the connection of a given syllable with those following it

in the series. This takes place through mediating associations of the intervening syllables.

Substitution is another important factor in false cases. For instance, if an idea *a* tends to reproduce idea *b*, an idea similar to *a* will also tend to reproduce *b*. This is active substitution. Passive substitution is the reproduction of an idea similar to *b*, on giving the stimulus *a*. Thus a syllable *a* may show a tendency to reproduce a syllable *c* which neither mediately nor immediately follows *a*, but only possesses the peculiarity of being associated with a syllable *b* which is itself associated with *a*.

Further causes of false cases, having close connection with the reproduction tendencies just mentioned, are the following:

1. Syllables given a week or a month before are recalled instead of the right one

2. When a given stimulus does not reproduce the right syllable it still brings it to the threshold of consciousness, so that it comes suddenly into consciousness and is named when the next syllable is shown.

3. In a few cases the response to a given stimulus is a stimulus that has already been given.

4. The given syllable may be one that will, together with the shown syllable, give the sound of a familiar word, viz., *bur—dok*.

5. Syllables similar in meaning to the proper one are sometimes given.

It may here be noted that ease of learning is increased in both abnormal and normal cases.

1. If two or more successive syllables have the same initial consonant.

2. If two successive syllables form a rhyme.

3. If two successive syllables contain the same vowel or diphthong.

4. If the final consonant of a syllable is the same as the initial consonant of the syllable immediately following.

5. If two or more syllables form a word or phrase, or if the syllable is itself a word.

6. Unusual combinations and those especially difficult to pronounce, are a hindrance to learning.

7. Rhythm is favorable.

Difference of Distribution (Jost's Law)

A period of rest of greater or less length between the repetitions of the syllable series has an important and favorable influence on their recall.

According to Jost, "Of two associations of the same strength but different age, the more recent disappears quicker than

the older,"[1]—so that the number of right cases is inclined to be less when the recall takes place immediately after the reading, than when a period of rest ensues, giving the impressions time to *set*, as it were. Jost found that the associations which were obtained by a constant number of repetitions of a syllable series possessed greater strength twenty-four hours after the last repetition, if the repetitions were distributed over several days, with intervals of twenty-four hours.

Jost also shows that the proved advantage of the distribution with long intervals cannot be wholly explained through the fatigue which is experienced from a greater number of repetitions immediately following each other, and as an explanation he formulates the important law that when associations are of like strength but different age, a new repetition possesses a greater strengthening power for the old association than for the new, or, in other words, associations of the same reproducibility disappear more slowly the older they are and the more frequently they are strengthened by new impressions.

It is a significant fact that the results show the influence of distributed repetitions to be markedly favorable in pathological cases, much more so than in the normal. This is due to the fact that the mentally defective subject is unable to keep his attention on the constantly repeated series until the time of recall, while the series whose repetitions are distributed over several days is met with fresh attention each day, and is reinforced in the interval by involuntary recall.

This test was conducted with the double series, each consisting of ten double, or twenty single syllables. One such list was repeated once a day for ten consecutive days. The other was repeated ten times daily until perfect recall was possible. Only one complete test could be made, owing to the difficulty of being able to arrange ten consecutive days on which the subjects were not kept from the experiments by their physical condition, as every care was taken that this work should not interfere in any way with the treatment and improvement of the patients. Owing to this many results had to be thrown out, because more than the twenty-four hours would intervene between experiments.

TABLE VIII

Subjects.	No. distrib. repets. nec. for correct recall.	No. consecutive repets. nec. for correct recall.
F—(Cerebral Syphilis)	10	40
B— "	10	40

[1] Jost: *op cit.*, p. 459.

As in all the tests, only ten consecutive readings were allowed daily, so that the results under this heading are diminished by the three twenty-four hour intervals necessary to forty repetitions.

One would conclude from the results of this test that persons with defective memories would find it very beneficial when memorizing, to distribute the repetition over as great a length of time as poosible.

Some results were taken to show the advantage of connected over disconnected matter in facility of learning, and the advantage of learning as a whole over that of learning in parts, but they are not sufficiently numerous or definite to offer very important explanations.

In conclusion, I desire to thank Dr. A. M. Barrett, Director of the ward, for permission to use the patients, for access to the case records and numerous suggestions in the course of the experiments.

Summary

1. Increasing the number of syllables in a series increases the number of repetitions required in the case of these insane patients much faster than in the case of the normal individual.

2. Generative, effectual and retroactive inhibition are much more pronounced in their effect in the abnormal than in the normal.

3. There is much greater advantage in divided over 'heaped up' repetitions for the insane than the sane.

4. All the sources of confusion and error work more markedly for the pathological mind than for the healthy.

In short all the factors investigated are operative in the diseased as in the well mind, and with much greater force.

THE INTERMITTENCE OF MINIMAL VISUAL SENSATIONS

STUDIED FROM THE SIDE OF THE NEGATIVE AFTER-IMAGE

I

THE FLUCTUATION OF THE NEGATIVE AFTER-IMAGE

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I. Introduction

This paper is a continuation of a study published in this *Journal* in January, 1906.[1] In that article the fluctuation of minimal cutaneous and minimal visual stimuli was discussed. The absence of fluctuation for cutaneous stimuli was demonstrated by a series of experiments, upon a number of observers, with liminal pressure and electrical stimuli.[2] The fluctuation

[1] C. E. Ferree: An Experimental Examination of the Phenomena usually Attributed to Fluctuation of Attention: XVII, 81.

[2] This part of the work has recently been repeated and extended by L. R. Geissler (Fluctuations of Attention to Cutaneous Stimuli, this *Journal*, XVIII, 1907, 309), and the previous results are confirmed. On the score of method it may be noted that a very thin, narrow strip of high-grade wrapping foil does better service than Christmas tree foil, and that the tongue should not be protruded, but allowed to lie

of minimal visual stimuli was explained as a phenomenon of adaptation. The thesis was maintained, first that adaptation is rendered intermittent chiefly through the influence of involuntary eye-movement; and secondly that eye-movement interferes with adaptation in two ways: it reduces the time of stimulation, and by shifting the retina into a region of different stimulation, it causes the restoration of the adapted elements.

The first of the effects just mentioned is obvious; but the second demands explanation. That eye-movement does restore the adapted retina can, in the present state of our knowledge of the phenomenon, scarcely be questioned;[1] but why a single eye-movement, consuming but a fraction of a second, or even a group of eye-movements, is able to restore a color or brightness that required a much longer time for adaptation, is not readily understood and needs to be investigated. Both the fact and its explanation have been the subject of much discussion in the history of visual theories. This discussion will be touched upon only briefly. It is not our purpose here to give a detailed account of visual theories. That will be attempted in a later paper. The object at this time is briefly to state such phases of visual theory as will be sufficient to introduce our problem. We find, in general, two main types of theory: the one represented by Fechner,[2] Helmholtz,[3] Fick and Gürber, etc., called the theory of fatigue; and the other represented by Plateau,[4] Hering,[5] G. E. Müller,[6] and others, usually called the theory of antagonistic visual processes, or, when the after-effect of stimulation is more especially regarded, the oscillatory theory.

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naturally upon the floor of the slightly opened mouth, with its edges pressing lightly against the lower teeth and lip.

[1] See Fechner, Pogg. Ann., XLIV, 1838, 525; Helmholtz, Physiol. Optik, 1896, 510; Fick and Gürber, v. Graefe's Archiv, XXXVI, 2, 1890, 246; Hess, v. Graefe's Archiv, XL, 1, 1894, 274; MacDougall, Mind, XI, 1902, 316; XII, 1903, 289; C. E. Ferree, this *Journal*, XVII, 1906, 79-121.

[2] Pogg. Ann., XLIV, 1838, 221, 513; XLV, 1838, 227; L, 1840, 193, 427. The theory was conceived earlier by Scherffer (Abhandlung von den zufälligen Farben, Wien, 1765; also Journal de Physique de Rozier, XXVI, 175, 273), who explained the negative after-image by the diminished sensitivity of the fatigued retina.

[3] Müller's Archiv f. Anat. u. Physiol., 1852, 461; Pogg. Ann., LXXXVII, 45; Philos. Mag., (4) IV, 519; Physiol. Optik, 1896.

[4] Ann. de Chimie et de Phys., LIII, 1833, 386; LVII, 1835, 337; Pogg. Ann., XXXII, 1834, 543. More fully in Essai d'une théorie générale, etc., Mem. de l'Acad. de Belgique, VIII, 1834. For a still earlier form of the theory, see de Godart, Journal de Physique de Rozier, VIII, 1776, 1, 269.

[5] Zur Lehre vom Lichtsinne, 1874; v. Graefe's Archiv, XXXVII, 3, 1891, 1; XXXVIII, 2, 1892, 252.

[6] Zur Psychophysik der Gesichtsempfindungen, 1897 (off-printed from Zeitschrift f. Psychologie, X, 1896, 1, 321; XIV, 1897, 1, 161).

The oscillatory theory, in general, does not attribute to eye-movement any direct influence upon adaptation. As formulated by Plateau, 1833-35, it was primarily intended to explain the occurrence of the positive and negative after-images, the fluctuation of the negative after-image, and the flight of colors. Every light stimulation arouses two processes in the retina: the one corresponding to the positive visual impression, and the other to the negative after-image. When the eye has been stimulated by light and the stimulus is removed, the retina attains to its normal state only after a series of oscillations between these opposing processes. When the after-effect of stimulation is observed in an illuminated field of vision, the positive phases of the oscillating processes are obscured by the general illumination. Hence we have the phenomenon of the negative after-image and its fluctuation (the phases of invisibility of the negative after-image corresponding to the recurrence of the obscured positive after-image). When, however, the observation is made in a darkened field of vision, all the changes are noticeable. The changes thus observed in after-images of certain brightly luminous stimuli have been denominated the 'flight of colors.'

The phenomenon of adaptation was not discussed in this early form of the theory. It was taken into account only when the cardinal features of Plateau's theory (the antagonistic nature of the visual processes and the oscillatory after-effect of stimulation) were later made the basis of a group of visual theories, of which the Hering theory may be taken as the type. These theories were called upon to explain not only how the eye becomes adapted to a given stimulus, but also how, when once adapted, it is restored to its normal state. In general, they account for the restoration of the adapted retina by the conception of antagonistic processes; every visual process carries with it its corrective process. All factors extrinsic to the processes themselves are declared to be unimportant. It is said, *e. g.*, that the eye does not become permanently adapted to a given set of stimuli, or condition of illumination, first, because of the tendency toward correction inhering in the nature of the opposing processes, and, secondly, because the stimulation of the eye is continually changing, owing to changes in position of body and head, movement of eyes, blinking, etc. But these latter factors are on no account to be considered as exerting a direct influence on the retinal processes. They merely give the corrective tendency, inhering in the sets of processes, a better chance to operate.

It will be seen that the above mentioned provision explains the relief of adaptation only in a general way, and also inadequately. For, without taking into consideration extrinsic in-

fluences, it cannot account for the comparatively large measure of restoration brought about in the short intervals of relief from stimulation afforded by the involuntary eye-movements which occur under the conditions of normal fixation, or by voluntary eye-movements, of short duration, away from the stimulus and back again. The intervals are much too short for correction to take place, to such a degree, of its own accord.

As these disturbances in adaptation cannot be explained, they are denied to be of any considerable importance. Involuntary eye-movement under the conditions of normal fixation, or voluntary eye-movement away from the stimulus and back again, is said to exert no noticeable effect upon the restoration of the adapting stimulus, or upon its obverse aspect, the negative after-image.[1] The interval, during which the retina is shifted away from a given stimulus, must in point of time alone account for the progress it has made towards regaining its normal condition.

On the other side, however, the representatives of the theory of fatigue attribute a direct influence to eye-movement in restoring the stimulated retina to its normal state. This influence is inferred, more particularly, from the effect of eye-movement upon the negative after-image. The argument is as follows. A voluntary eye-movement, or a noticeable involuntary eye-movement, causes the after-image momentarily to disappear. The negative after-image may be taken as the index of retinal fatigue; hence whatever is able thus profoundly to disturb the cause of the after-image must also function in the recovery of the fatigued retina.[2]

According to Fechner (*opp. citt.*) eye-movement causes the after-image to disappear because of mechanical disturbances in vascular and nervous influences on the retina: temporary vascular congestion, etc. Helmholtz[3] says that eye-movement causes the after-image to disappear by producing changes in the illumination of the retina. Both writers, apparently, have practically disregarded the effect of eye-movement upon the total duration of the after-image; although, as will be shown later, this must be considered a much more important factor in a study of the restoration of the adapted retina than are the momentary disappearances or fluctuations of the after-image.

Of the more recent writers, Fick and Gürber contend that eye-movement changes the lymph-stream in a way that facilitates the removal of the fatigue products and the delivery of

[1] Hering: *opp. citt.*
[2] See especially Fick and Gürber, v. Graefe's Archiv, XXXVI, 2, 1890, 246.
[3] Physiol. Optik, 1896, 510.

new material to the fatigued areas. Hering,[1] replying to Fick and Gürber, denies to eye-movement any peculiar power to relieve adaptation. He asserts that movement of the field of vision, for example, answers the purpose equally well. Hess (*op. cit.*) contends that an adapted stimulus, steadily fixated, is not recovered, but does not explain how eye-movement restores the adapted retina. Finally, MacDougall (*opp. citt.*) explains the effect of eye-movement upon the reappearance of minimal visual stimuli on the basis of innervation.

This is the condition in which we find the problem at the present time. The oscillatory theory makes no provision for any noticeable effect of eye-movement upon adaptation, nor can it explain the after-image results which we ourselves have obtained. Fechner and Helmholtz ascribe to eye-movement a direct influence upon adaptation, but their hypothesis as to the way in which this effect is produced can be shown to be untenable. MacDougall's position has already been discussed by the writer;[2] while Hering, as will be shown later, did not carry his observations far enough.[3]

While, however, the literature does not furnish a satisfactory solution of the problem, it strongly suggests a method of investigation. It is evident that we cannot adequately study recovery while the stimulus is acting. We can only note the coincidence of eye-movement, or what not, with recovery. What goes on, in the small interval for recovery afforded by a single eye-movement, defies observation or experimental analysis. Fortunately, however, the after-effect of stimulation affords an easy and obvious point of attack. Here we can study recovery in isolation, and may hope to determine the factors that influence it: the factors that cause the fluctuations and affect the duration of the after-image. In accordance with this plan, a series of experiments on the negative after-image was begun in the Cornell University laboratory in the spring of 1904. The results of these experiments will form the subject-matter of this and the following papers.

The material may be classified under the following heads: I. Relation of the negative after-image to adaptation; II. Fluctuation of the negative after-image; III. Duration and fluctuation of the negative after-image with reference to its bearing upon the intermittence of minimal visual stimuli. This, the logical order of treatment will, however, be changed for the sake of convenience of discussion. The determination

[1] v. Graefe's Archiv, XXXVII, 3, 1891, 22.
[2] This *Journal*, XVII, 1906, 89.
[3] Fick and Gürber's hypothesis, although too indefinite and too speculative, seems to be the most promising of any of the historical hypotheses. It will be discussed later.

of the relation of the negative after-image to adaptation depends, in part, upon the results of the two succeeding inquiries, and can therefore be most conveniently taken up at the end of the series. We shall, accordingly, begin with a discussion of the cause of the fluctuation of the after-image, and of the factors influencing its duration. In terms of theory, these must constitute the factors that work for the restoration of the stimlated retina; for whatever theory is held of the adaptation and after-image phenomenon,—whether it be ascribed to fatigue, to antagonism of retinal processes, or what not,—the factors that work against the after-image operate to restore the stimulated retina to its normal condition. To make our position more secure, however, we shall report, in a second paper, adaptation experiments which show that the experimental variations that increase the frequency of fluctuation of the after-image and decrease its duration increase the time required for a stimulus to adapt; and, conversely, that the devices that decrease the frequency of fluctuation and increase the duration of 1 the after-image ~~increase~~ the time required for a stimulus to adapt. 1 decrease The tests thus established will then be applied to the fluctuation of liminal stimuli. It will be shown that whatever increases the fluctuation of the after-image and decreases its duration, increases the phase of visibility and decreases the phase of invisibility of the liminal stimulus; and conversely, that whatever decreases the fluctuation of the after-image and increases its duration, decreases the phase of visibility and increases the phase of invisibility of the liminal stimulus; as should be the case, if the fluctuation of liminal stimuli is an adaptation phenomenon.[1] Thus the chain of identification will be rendered complete. The intermittence of minimal visual stimuli will have been made to answer to the tests for adaptation, from both its obverse and its reverse sides. Moreover, in the progress of the work. an answer will be found to the question how eye-movement is able to relieve adaptation.

The whole course of the work which we have undertaken on the fluctuation of minimal visual stimuli may be summarized as follows. First, an examination of the phenomenon was made for the ascertainment of its possible causal factors. These were found to be disturbances in accommodation; adaptation, which is found to be intermittent with normal fixation; fluctuation of attention; and physiological disturbances in the visual centre due to the function of other brain centres, such, for

[1] This result follows directly from the adaptation experiments just referred to. For whatever increases the fluctuation and decreases the duration of the negative after-image, increases the time required for a given stimulus to adapt (the phase of visibility when the stimulus is liminal), and so on.

example, as the respiratory and circulatory centres. Secondly, we eliminated, by experimental process, all of these factors with the exception of adaptation. Thirdly, we have been able to identify fluctuation with intermittent adaptation from both its obverse and its reverse sides. And fourthly, we have determined the factors that disturb adaptation.

II. The Fluctuation of the Negative After-Image.

There is a strong interest in the fluctuation of the negative after-image, independently of its bearing upon our special problem. It is generally recognized as one of the important problems in psychological optics, and one not as yet adequately taken into account by visual theories. We find, for example, Plateau, Aubert, Hering, Ebbinghaus and others holding that periodicity is grounded in the nature of the after-image process; the followers of Fechner and Helmholtz contending for accidental influences of various kinds which operate upon the fatigued retina; and Exner maintaining that eye-movement causes the after-image to disappear, because it distracts from clear perception. The question, then, is still open. The evidence is such that, unless prejudiced in favor of some particular explanation, one cannot subscribe to any without further investigation. Thus, von Kries, writing in 1905, testifies to the lack of decisive results as follows: "Die Frage, ob das Schwinden einer lokalen Umstimmung sich überhaupt in dieser Form eines allmählichen Abklingens vollziehe, ist (ohne messende Versuche) viel diskutiert und mehrfach in verschiedenem Sinne beantwortet worden." [1]

i. *Hering and Exner.*

In our consideration of the various theories, those which deny causal relation between eye-movement and fluctuation, Hering's and Exner's, will be examined first. The hypotheses which seek to explain the effect of eye-movement on fluctuation, Fechner's, Helmholtz' and Fick and Gürber's, will be deferred until a later point in the inqniry. It is convenient to begin with Hering.

a. Hering's discussion of the effect of eye-movement on the fluctuation of the negative after-image (1891) grew out of a controversy regarding the effect of eye-movement upon the restoration of the fatigued retina to its normal condition. In this discussion, he has not kept the two subjects formally separated, so that there must be more or less cross-reference between them in our review; although the centre of interest for us, at this stage, is the fluctuation of the negative after-image.

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[1] Nagel: Handbuch d. Physiol. des Menschen, III, 216.

The whole question of the influence of eye-movement on the visual processes was raised by the representatives of the theory of fatigue. They sought to explain first why the eye, fatigued by a particular stimulus, recovers as quickly as it does; and, secondly, why it does not become progressively fatigued by light stimulation in general during the twelve hours or more of its exposure to light in the course of a day. The explanation was given chiefly in terms of the changes brought about in the fatigued retina by eye-movement. As has been stated above (p. 62), Fechner asserted that these changes are of the nature of mechanical disturbances in vascular and nervous influences; while Helmholtz attributed them to the more or less continual changes in the illumination of the retina due to eye-movement in connection with blinking, frowning, etc. Obviously, neither hypothesis is adequate to explain the facts in question.

Fick and Gürber (1890), taking up the problem at this point, were concerned first to furnish a more extended experimental demonstration of the fact that eye-movement is effective to relieve the fatigued retina; and secondly to explain, more adequately than had been done by Fechner and Helmholtz, how this relief is accomplished. The first point will not be discussed here. The explanation may be summarized as follows: eye-movement restores the fatigued retina by influencing the metabolic changes that take place in the fatigued area; it both facilitates the removal of the fatigue products from this area, and augments the delivery of new nutrient material to it.

Replying to Fick and Gürber, Hering denies that eye-movement affects the visual processes, and explains the absence of progressive fatigue by his conception of assimilative and dissimilative processes which are mutually corrective. He bases his denial that eye-movement is a factor in restoring the stimulated retina to its normal condition upon four experimental proofs, all adduced to show that it neither causes the negative after-image to disappear nor produces any other noticeable disturbance in its temporal course. These four proofs are as follows. (1) After continued fixation of a stimulus, the after-image does not disappear when the eyes are rapidly moved away from the stimulus and back again. (2) Movement of the background causes the after-image to disappear; hence eye-movement can possess no peculiar power to produce its disappearance. (3) Near-lying after-images due to successive stimulations do not fluctuate together. (4) Eye-movement does not cause the after-image to disappear, when it is observed in a darkened field of vision. These points will be taken up in the order given.

(1) The demonstration is as follows. If a disc or square of

dead black paper is laid on a white background, and its centre fixated for some time; and if the observer then moves his eyes quickly out to some near-lying point and back again to the neighborhood of the stimulus; the after-image is not found to have disappeared as a result of the movement.

Hering does not draw any specific conclusions from this experiment alone. It will be remembered, however, that by means of it and of the succeeding experiments he wishes to establish the thesis that eye-movement does not cause the disappearance of after-images, or otherwise noticeably interfere with their temporal course, and that it does not factor in the restoration of the fatigued retina. It seems fair to add, as an obvious corollary to this thesis, that it does not cause the fluctuation of the negative after-image. Now it is evident that the experiment is of little or no value in the present connection. (*a*) For to conclude from it that eye-movement does not cause after-images to disappear would be to generalize from a very special case, namely, from an after-image of high intensity. The result is very different when the after-image is weaker; eye-movement readily brings less intensive after-images to disappearance. In general, after-images obtained with so long or even with a less long period of stimulation must dim to some extent (the amount depending upon the time of stimulation) before eye-movement can cause them to disappear. (*b*) To conclude from it that eye-movement does not factor in the restoration of the fatigued retina to its normal condition would be to apply a test that is over-strict. It is not necessary that the after-image disappear. A dimming of the after-image should indicate partial restoration of the fatigued retina. In fact, the writer has shown in the rough, by a series of experiments to be described in a later paper, that the restoration of the adapted retina is proportional to the loss of intensity in the after-image. The disappearance of the after-image corresponds to complete restoration of the adapted retina, and should not be required as evidence that partial restoration has taken place. To demonstrate, then, that eye-movement factors in the restoration of the retina, it need be shown only that the after-image has lost in intensity; and proof of this is easy, however strong the stimulation. Observations made with reasonable care give the uniform result that after-images, of whatever intensity, are dimmed by eye-movement. (*c*) To conclude from it that eye-movement is not a causal factor in fluctuation would be to ignore certain relevant facts. After-images of such intensity do not fluctuate. Just as they must dim, to some degree, before voluntary eye-movement can cause their disappearance, so must they dim before fluctuation begins. If, therefore, the argument from

analogy is to be used at all, the investigator must first determine at what intensity after-images begin naturally to fluctuate, and at what intensity voluntary eye-movement of a suitable range begins to cause disappearance; and may then ask whether a rough correspondence obtains between the two points. This procedure was followed by the writer with a number of observers; and the results show, uniformly, an exceedingly close correspondence. A description of the method used and a statement of the results are given further on, p. 103 ff. Obviously, then, nothing can be derived from this experiment that will aid in demonstrating either the Hering thesis or its corollary.

(2) In his second observation, Hering is concerned to disprove Fick and Gürber's theory that eye-movement facilitates metabolic change in the retina. Even if disappearance does follow movement of the eyes, he says, it is not necessarily implied that eye-movement possesses any peculiar power to cause disappearance; for movement of the background yields the same result. The effect of moving the background is explained as follows: "Dies hat seinen Grund in der Wechselwirkung der Sehfeldstellen und zum Theile auch darin, dass die Augenmedien nicht ganz homogen sind und daher immer mehr oder weniger Licht von der Bahn abirrt, die wir ihm theoretisch zuschreiben." The explanation would seem to give up the whole controversy; for, as it stands, precisely the same effect should be produced by moving the eyes as by moving the background. However, we let this point pass, and proceed to consider the statement that movement of the background causes disappearance. That is true. It is possible, within limits, to duplicate, by movement of the background, any fluctuation series that may be produced by voluntary eye-movement. In all such cases, however, the eye is tempted to movement by the moving background. At any rate, when the eye is held steady, movement of the background does *not* cause the after-image to disappear. This may be demonstrated as follows. (*a*) Use for the background the mottled surface presented by the darker side of engine-gray cardboard. Project the after-image. Let it become sufficiently dim, and move the cardboard in any direction. Disappearance takes place. Now, place a fixation point immediately in front of the background; *e. g.*, a black knot in a taut vertical white cord. Fixate this steadily. Movement of the background scarcely dims the after-image. (*b*) The following method is probably not so fair a test of Hering's position as that just described, since he asserts that a change in the illumination of any part of the retina acts reciprocally on other parts. Hence the maximal effect would be produced, we may suppose, by movement of the whole background, and not by movement of the particular area

upon which the after-image is projected. However, the facts may speak for themselves. Use the same mottled engine-gray cardboard for the background. Place just in front of it a sheet of same kind of cardboard, with a hole of the exact size and shape of the after-image to be observed. Looking through this hole, project the after-image upon the background. Move this in any direction. Now that the major portion of the field of vision is steady. the shifting of the area upon which the after-image is projected does not noticeably disturb fixation, and correspondingly does not cause the after-image to disappear. (Instead of a sheet of cardboard, a disc mounted upon a color-mixer may be placed behind the opening. When the after-image is projected upon it, the disc may be rotated at any chosen rate of speed without sensibly dimming the image.)

(3) Hering's third demonstration is as follows. Place a short, broad strip of colored or dark paper on a white background 5 mm. to the left of a fixation point. Observe for 10 sec. Quickly remove it and place a similar strip, parallel to the position of the first, 5 mm. to the right of the fixation point. Observe for 10 sec. Remove this, and replace the first strip for 10 sec. Then remove the first, and replace the second strip for 10 sec. Thus the eyes have been exposed to both strips for 20 sec., with an intermission for each of 10 sec. The object of this arrangement was, apparently, twofold: first, by successive stimulation, to start the after-images in different phases of oscillation, and thus to cause them to fluctuate successively; and secondly, by causing them to fluctuate successively, to show that eye-movement cannot have been responsible for their fluctuation, but rather that oscillation is grounded in the nature of the visual processes. With regard to the first point, however, it can be shown that there is no especial virtue in successive stimulation to produce successive fluctuation in after-images situated on different parts of the retina. If two stimuli, not too large, are placed at a certain distance apart and allowed to act simultaneously, their after-images rarely fluctuate together. This is one of the common phenomena of fluctuation, whatever the temporal character of the stimulation, and it is in nowise essentially dependent upon successive stimulations. With regard to the second point,—that if eye-movement had anything to do with the fluctuations, the after-images should have fluctuated together, and not independently of each other,—we urge that the conclusion does not by any means follow from the premisses. It is true, as is pointed out by Hering, that both areas of the retina had been stimulated for the same length of time; and that, so far, the after-images should have been affected alike by eye-movement. But Hering overlooks the fact that the one after-image had been fading for 10 sec.

before the stimulus to the other was removed. It had thus run a large part of its course before the other began, and hence might be expected to disappear under a range of movement that would scarcely dim the other. We cannot only say that eye-movement may have been the cause of the independent fluctuation, but we can go farther and say that the after-images behaved precisely as they should have done if eye-movement were the cause of their fluctuation.

But farther, Hering's result, as well as his conclusion, must be called in question. The writer has tried the experiment upon himself and a number of observers, and so far from finding Hering's results invariable or even typical, has rarely met with a case in which, after the first couple of fluctuations, the one strip disappeared as a whole while the other remained intact. Instead of that, the whole area formed by the two broad strips and the narrow contrast strip between fluctuated, either as a whole or in parts, as all after-images of a certain magnitude do. When the fluctuation was in parts, now a corner of the area would drop away, now a strip across the top, now an irregular patch here followed by an irregular patch there, etc., etc. When the area fluctuated as a whole, first the two outside strips would spread over the intermediate space, the whole area becoming dim in consequence; then the entire image would disappear. When observation was made on the closed lids, this experiment furnished an excellent demonstration of the relation of the 'streaming phenomenon' to fluctuation.[1]

Hering's arrangement failed to give successive fluctuation for the reason that the strips were too large for their distance apart. In this zone of the retina, the zone of fluctuation in parts, the area included in each disappearance was not large enough to include the whole strip. Had the strips been placed farther apart, or made smaller and placed as Hering directed, the successive fluctuations aimed at would have been uniformly obtained; but their occurrence it is plain, would in no wise have demonstrated the intrinsically oscillatory character of the underlying visual processes. Still better results, however, would have been obtained if the stimuli had been smaller and also placed farther apart. A square or rectangular after-image, large enough to include the strip-areas, would also have fluctuated successively in parts, the fluctuating area now and then corresponding to, or including, the two strip-areas, in turn. All these fluctuation phenomena are due to variation in the

[1] For a description of this phenomenon, see below, p. 114, and for an explanation of its relation to the after-image, p. 123. The stream could be plainly seen to diffuse the color or gray of the two outside strips over the intermediate strip, and finally to blot out the whole image.

area of the retina involved, and have nothing to do with successive stimulation. Over a certain range of areas, fluctuation in parts is the invariable occurrence, whatever the temporal character of the stimulation.

We may now sum up the discussion of this experiment. So far from showing by his special device of successive stimulations that eye-movement cannot be the cause of fluctuation, and so far from throwing any difficulty in the way of the eye-movement hypotheses, Hering has succeeded rather in making it easier to explain, by eye-movement, the results which he obtained. In other words, he has produced the phenomenon of fluctuation in parts in the only way, known to the writer, in which it admits of ready explanation by the theories of Fechner, Helmholtz, and Fick and Gürber. In terms of any eye-movement hypothesis, the difference in intensity of the two after-images is amply sufficient to explain why a given eye-movement should not affect both of them alike. Hering's results would have been more difficult to explain had he used simultaneous stimulation. It is much more difficult, *e. g.*, to say in terms of eye-movement why after-images of a certain area fluctuate in parts, or why small after-images due to simultaneous stimulation of different parts of the retina fluctuate independently of one another. The fluctuation in the case both of simultaneous and of successive stimulation is, however, of the same nature, and its cause must, evidently, be assigned upon other grounds than those here given by Hering.

(4) As a final step in his argument, Hering maintains that, in order to give a final decision of the question whether eye-movement exerts any influence upon the disappearance of the after-image other than by causing changes in the illumination of the retina, the course of the after-image must be traced in a darkened field of vision. He maintains that under these conditions eye-movement does not noticeably alter the natural course of the after-image, much less cause it to disappear. A few sentences further on, however, he qualifies this statement by the remark that, when observing in a dark-room, he was never able entirely to blot out an after-image that was at all distinct or intensive, by moving the eyes.[1]

[1] "In den ersten Paragraphen meiner Mittheilungen zur Lehre vom Lichtsinne habe ich eine Reihe von Erscheinungen besprochen, welche man an Nachbildern im geschlossenen und verdunkelten Auge beobachtet. Ich hatte bei solchen Versuchen reiche Gelegenheit festzustellen, dass Augenbewegungen den gesetzmässigen Verlauf dieser Nachbilder gar nicht merklich beeinflussen. Auch habe ich zahlreiche Versuche in einem Zimmer angestellt, welches völlstandig verdunkelt werden konnte, nachdem ich mir das Nachbild erzeugt hatte. Hier hatte ich den Vortheil, die Augenbewegungen bei ebenfalls offenen Augen ausführen zu können. Nie war es möglich ein irgend

Apparently, there is here a tacit admission that eye-movement causes weaker or less distinct after-images to disappear. If so, the argument against it as a causal factor is materially weakened; for, as has been stated earlier in the discussion, intensive after-images do not fluctuate at all. They must become sufficiently dim if fluctuation is to set in. In any event, however, observation is the court of final appeal. We must determine, first, whether eye-movement does cause after-images, weak or strong, to disappear when they are observed in a darkened field of vision; and secondly, whether the point at which fluctuation begins roughly coincides with the point at which eye-movement first causes the after-image to disappear. A series of experiments was conducted to this end. Many stimuli were used, colored and gray, and the after-images were observed under the following conditions: with the eyes closed and carefully covered by a black cloth; in the dark-room, with the eyes both open and closed; and in the blackness cylinder. In every case eye-movement caused disappearance when the after-image had become sufficiently dim, and this point roughly corresponded with the point at which fluctuation began. The following results, which are typical, have been selected for publication. In this case the stimulus was a square of Hering white paper, 5 by 5 cm., on a background of Hering gray no. 31; and the after-image was observed with the eyes closed and covered with a black cloth. Miss Alden, a graduate student in psychology in the University of Colorado, acted as observer. The time of stimulation was 40 sec., and the distance of the observer from the stimulus was 75 cm. The recording apparatus consisted of kymograph, telegraph key and electro-magnetic recorder, and electro-magnetic time-marker in circuit with a small chronometer set to half-seconds. When the disappearances were caused by eye-movement, the eyes were moved every 3 sec., at a signal from the experimenter. It may be well to add that the results showing the closest approximation have not been chosen for publication. In the case selected, too much eye-movement was prescribed. The after-image began to fluctuate at a greater intensity than in the companion series of natural fluctuations. There were more frequent fluctuations, and the average phase of visibility was shorter. Erring as they do, however, on the side of making eye-movement too effective, these results tell more strongly against Hering's assertion that eye-movement does not cause disappearance in a darkened field of vision than do the results

deutliche Nachbild durch Augenbewegungen, auch wenn sie ungewöhnlich gross und lebhaft waren, zum verschwinden zu bringen." In v. Graefe's Archiv, XXXVII, 2, 1891, 23.—*Cf.* also S. Exner, Zeits. f. Psychol., I, 1890, 47.

which show a closer approximation. For this reason, and also because about the same amount of eye-movement was prescribed as in the other duplication experiments which had already been carried out, this particular series has been selected.

TABLE I

A. Showing results for a darkened field of vision when the fluctuations were natural, and when they were produced by voluntary eye-movement. Unit 1 sec.

TYPE OF FLUCTUATION	NO. OF FLUCT'S	1st VIS.	AV. VIS.	TOTAL VIS.	AV. INVIS.	TOTAL INVIS.	VIS. + INVIS.
Natural	7	9.9	6.6	46.2	2.2	15.4	61.6
Produced by Voluntary Eye-movement	8	6.2	4.8	38.4	2.5	20.0	58.4

With regard to the natural fluctuation of after-images, when observed in a darkened field of vision, Hering says: "An den ersterwähnten Nachbildern (*i. e.*, those due to a bright object seen on a dark ground, fixated for 10-30 sec.) aber erfordert es sogar besondere Aufmerksamkeit wahrzunehmen, dass das negative Nachbild nach längerem Bestehen nicht bloss vorübergehend verschwindet, sondern dass sich zwischen sein Verschwinden und eventuelles Wiedererscheinen eine schwache positive Phase einschiebt, die freilich oft genug überhaupt nicht merklich wird."[1] It is difficult to determine whether this statement means that the weak positive phase occupies all of what usually passes for the phase of disappearance, or only a part of it. If it occupies the whole time, there is of course no intermission in the after-image process. The disappearance usually observed is merely an artifact, produced by observation with the retina illuminated. In view of this uncertainty it seemed worth while to repeat the experiments. Hering says that the recurrence of the positive phase may be observed if one fixates a bright object on a dark ground for 10, 20, or 30 sec., and then watches the after-image in a darkened field of vision. A square of white paper on a dark ground was taken as stimulus. This gave, as negative after-effect, a black square with a distinct contrast border of brilliant white on a very light gray ground. The black square fluctuated frequently; but there was never left in its place, nor did there ever appear anything that resembled, a square of white or light gray. When it disappeared, however, some part of the contrast border at times remained momentarily visible and often could be seen to

[1] *Op. cit.*, 18.

reappear slightly in advance of the black square;[1] but this phenomenon could scarcely be mistaken for a weak phase of the positive after-image. A small, irregular patch of slightly luminous haze is also frequently noticed about the point of regard.[2] But this occurs just as frequently during a phase of invisibility when the eye has been exposed to a colored or white stimulus, or in the darkened field of vision when the retina has undergone no local stimulation at all; hence it cannot be a positive after-effect of stimulation.

In order to make the test-conditions still more favorable to Hering, we substituted for the dark gray background prescribed by him a light gray (Hering no. 15). Under the original conditions, the positive phase must have been difficult to distinguish, had it occurred. The negative after-effect now obtained was a black square upon a ground of gray such that not only would the lighter positive phase have been easily distinguishable, if it occurred, but that it would also have been considerably intensified by contrast. Still the positive phase could not be detected during the periods of invisibility of the negative phase.

When the stimulus is luminous, Hering says that the positive phases are plainly present, alternating with the negative. As before, two experiments were made upon this point by our observers: the one with the background light, but not so bright as the stimulus; and the other with the background dark. In the first experiment, the sun's disc and Colorado daylight served as stimulus and background. The bright background intensified the darkened field of vision in the after-effect, and thus, as far as brightness was concerned, favored by contrast the observation of the positive phase of the after-image. The eye was stimulated, probably, from 1 to 3 sec. The observations were made in mid summer, near the middle of the day. The sky was cloudless, and the light very intensive. While, now, it is difficult to decide what is positive, and what negative, in the color changes that follow exposure to a brightly

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[1]This, of course, is merely an instance of fluctuation in parts. The area that was swept out by the stream, or complex of streams, did not at first include this particular part of the contrast border; but soon, owing to the spread of the area of commotion, the contrast border became involved in the disappearance. The reappearance of a part of the border in advance of the remainder of the after-image is a phenomenon of the reverse order. The border was cleared of the streaming material before the rest of the image.

[2]Close observation shows this to be a centre of activity of a 'streaming' area. It also occurs, but less noticeably, in other parts of the field of vision more remote from the point of regard. Such patches are usually found to be the foci or places of intersection of narrow, swiftly moving streams.

luminous stimulus, still our observers reported, here as in the former experiments, unquestionable, well defined phases of disappearance. Voluntary eye-movement also produced disappearance when the after-image had reached the dimness at which fluctuation begins.

When exposed to the sun's disc, the eye was purposely moved in order that the after-image might be jagged and irregular. This was of some advantage for observation, because the limbs of the after-image, owing to their less intensive stimulation, passed through the color changes slightly in advance of the body. The after-image was observed with the eyes closed and covered with a black cloth. The following report is typical:

There was first a momentary lasting over of the stimulus in the body of the after-image, while the edges were a deep red. The body then changed to a light blue. The red border, in the meantime, had been gradually extending inwards, especially in the limbs of the image.[1] The edges of the central blue patch next began to change to a yellow-green. While this change was going on, the outer margin of deep red was encroaching, more and more, upon the centre. When the central portion had become almost entirely yellow-green, the marginal red had taken on a border of deep blue.[2] All the central portion next became yellow-green. The red border encroached still farther upon the centre, and was in turn encroached upon by its border of dark blue. The centre then showed a tendency to become light blue again. Finally, the whole image became a deep red with a dark blue margin. This stage lasted for a comparatively long time, and disappearances were frequent. Next, the dark blue of the margin spread gradually over the entire image. There were later a few faint recoveries (following disappearances) of a lighter blue; then came in order a light red-violet, a violet-blue, and a dull dark yellow. In these last fainter stages complete disappearances were especially frequent.

The fluctuations observed in this experiment were not different from those occurring under the usual conditions. The

[1] At this stage, there was a very noticeable effect of perspective. The red seemed to be projected farther into the background than the blue, farther back even than the general field of vision, as if it were sunk or seared into the field. The blue seemed a detached and floating patch, which was now and then swept away by a stream, changing its shape as it went, and dissolving in the stream body.

[2] These changes were not all gradual or continuous. Sometimes the central patch would change from light blue to the next stage, yellow-green, and back again; at times it would go from blue to the deep red, and back to blue again; and sometimes it would disappear entirely, frequently repeating in its reappearance all the color stages in their inverse order. Sometimes it would come back suddenly to the color from which it fluctuated; and, very occasionally, the order would be irregular. These changes were always connected with the streaming phenomenon. Light streaming, apparently, caused the changes from color to color, while heavy streaming blotted out the image entirely. Recovery came with the clearing away of the streaming material.

after-image fluctuated as a whole, and in parts, as all after-images do. Now one of the limbs would drop off; now the lower part, now the upper; now the image would disappear across the centre; and again it would disappear completely. In fact, this experiment, so far from furnishing evidence against fluctuation, gives (owing to the long duration of the after-image) an unusually good demonstration of the various phenomena that characterize fluctuation.

Experiments were also made with a dark background. Here, as before, observation showed clearly marked periods of disappearance. The intermission was absolute. No part of the interval was occupied by anything that could be identified with a recurrence of the positive phase at low intensity. Further details appear needless, as the conditions already described were more favorable than those of the dark ground for the observation in question.

It is logically impossible, then, to conclude from the foregoing experiments, as Hering does, that eye-movement is ineffective in the disappearance of the after-image, or that fluctuation is merely the alternation of negative with weak positive phases of the after-image. It is obvious, rather, that eye-movement is able to cause the diappearance of any after-image that will fluctuate.—

b. Exner argues against the view that intermittence is grounded in the nature of the after-image process. It is a well-known fact, he says, that eye-movement will cause an after-image to disappear. Nor does eye-movement affect the after-image process. The eye, moving in some particular direction, causes the field of vision to travel across the retina in the opposite direction. This moving field of vision, by distracting from the perception of the aftêr-image which is stationary upon the retina, causes it to drop out of clear consciousness. When the eye comes to rest, the distraction is removed, and the after-image reappears.[1] Now, this explanation accounts, at best, only for our inability to see the after-image while the field of

[1]Exner and Hering thus agree, though from different points of view, that there is no particular virtue in eye-movement to cause the disappearance of the after-image. Movement of the background works just as well. It is worthy of notice, however, that while Hering uses the statement as an argument for the oscillatory theory, Exner uses it as an argument against. Exner thinks it evident that movement of the visual field distracts from the clear perception of the after-image just as it distracts from the clear perception of objects actually in the external visual field; and explains the whole effect of movement of the background in this way. It is an easy step, then, to infer that the disappearances produced by voluntary eye-movement are to be similarly explained, and to refer the fluctuations occurring under the conditions of normal fixation to involuntary eye-movement rather than to oscillation of visual processes.

vision is in motion. It does not account for the invisibility of the image after the eye has come to rest. There are, however, two cases of this inability to see the after-image while the field of vision is in motion. In the one, the after-image is vaguely seen throughout, but cannot be seen clearly so long as the eye is in rapid movement. It comes out at once when the motion lags or ceases. This corresponds to Exner's distraction phenomenon; but it is not what is ordinarily meant by disappearance. The other is a case of true disappearance. The after-image goes out absolutely. It does not reappear as the motion lags, and is still invisible after the eye has accurately regained its fixation. In the writer's experiments upon fluctuations produced by voluntary eye-movement, a disappearance was not recorded unless the after-image remained invisible after the observer had accurately regained his fixation. Disappearances of this sort were evidently not due to distraction, for distraction had ceased before the record began.

Although Exner thus seems to be mistaken in his view of the disappearance produced by eye-movement, his theory will be put to experimental test. A direct corollary from it is that the effect of eye-movement upon the disappearance of the after-image bears an inverse relation to the uniformity of the projection field. There are three sets of conditions under which this relation should obtain: (*a*) disappearance under the conditions of ordinary fixation; (*b*) disappearance produced by voluntary eye-movement; and (*c*) disappearance caused by movement of the background. It certainly does not obtain under the first conditions. The after-image fluctuates with equal readiness when projected upon lettered surfaces, upon mottled engine-gray cardboard, upon either the dull or the glazed surface of milk glass (than which there is probably no more uniform background), and upon the Hering gray papers. Nor does it seem to make any difference which of the above backgrounds is used, when the disappearance is caused by voluntary eye-movement. The inverse relation does, however, seem to hold, within limits, when the disappearances are caused by movement of the background; the mottled backgrounds have, apparently, more effect than the uniform. Now it is evident that the mottled background, travelling across the retina, could distract no more from the perception of the after-image in this case than in the other two. It would, however, in proportion to its irregularity, distract from steady fixation. Thus the difference is to be explained in terms of increased eye-movement; and again the argument against eye-movement, upon more careful investigation, is converted into an argument for some sort of eye-movement hypothesis.

As the matter stands, then, with regard to Hering and Ex-

ner, eye-movement must still be taken into account in the explanation of the fluctuation of the negative after-image.

ii. *Demonstration of causal connection between eye-movement and fluctuation, as against the theory of intrinsic oscillation.*

a. Results in general.—In order that the thread of the argument may not be lost in the tables and details that follow, a brief general statement of results is here given.

(1) *Fluctuation occurs only within a limited range of after-image areas.* It is a matter of common laboratory report that fluctuation does not take place in the after-effect of general adaptation. The after-effect dies away gradually. There are none of the intermittent variations of intensity that characterize the after-effect of local adaptation to stimuli of certain areas. This fact is brought out in practically all the record-books kept by members of the junior training course at Cornell University. Careful tests have also been made, with the same result, by the help of observers trained to work with just noticeable differences, by whom even slight variations in intensity would have been noticed.

We turn to the after-effect of local adaptation. Here we find that fluctuation occurs only within a comparatively limited range of after-image areas, varying somewhat for the different colors used, and for different observers. Large after-images do not fluctuate at all; small after-images little, if at all; after-images of mean area alone fluctuate readily. If a curve of frequency were plotted with the areas laid off along the ordinate and the frequency of fluctuation along the abcissa, the curve would start close to the abcissa, rise gradually until a certain area was reached, and then bend down rather more sharply to the abscissa. This result is, apparently, incompatible with the hypothesis of intrinsic oscillation. Absence of fluctuation for a single area would tell strongly against that theory; and such a range of variation as is expressed in the curve of frequency would seem to condemn it absolutely. The shape of the curve of frequency, together with the fluctuation in parts of after-images of certain areas, is the most difficult problem that the fluctuation of the after-image presents to theory. That eye-movement, acting in co-operation with streaming, offers a satisfactory explanation of all the variations resulting from change in area will be shown in detail in its proper place.

(2) *Whatever renders fixation unsteady increases the frequency of fluctuation and decreases the duration of the after-image.* The converse is also true: whatever aids fixation decreases the frequency of fluctuation and increases the duration of the after-image. Various methods were used to disturb and to control fixation. In every case records of eye-movement were

taken, that showed both the range and frequency of the movements and the total time during which the eyes were in motion. A quantitative comparison could thus be instituted between these movements on the one hand, and the frequency of fluctuation on the other. The results show a high degree of correlation.

(3) *The form of the stimulus affects the frequency of fluctuation.* Experiments were made with squares and with narrow strips of equal area. The latter showed a much greater liability to fluctuation. This result can hardly be explained on the theory of intrinsic oscillation; the oscillatory character of the retinal processes must be sensibly the same within a square as within a narrow oblong area. There is, however, good reason to believe that eye-movement differs in the two cases; for when the strip is observed, the introspective reports of the observers bear witness to a strong conscious tendency to look towards the ends, to see what is happening there. The tendency to increased movement with the strip-images is shown also in the eye-movement records. Eye-movement, then, is a factor in the result. Another and, as we shall see later, a very important factor is the retinal distribution of the zones of streaming.

(4) *The arrangement of the stimulus with reference to the direction of greatest eye-movement affects the frequency of fluctuation and the duration of the after-image.* A strip after-image, placed with its breadth in the plane of the greater range and frequency of eye-movement, fluctuates more frequently and has a shorter duration than in the inverse arrangement. The greater frequency of fluctuation is due to the action of the greater amount of eye-movement upon the lesser dimension of the after-image. The point may be demonstrated as follows. Let the disappearance be produced by voluntary eye-movement. If these movements are in the horizontal plane, disappearance is more frequent when the breadth of the strip is in the horizontal than when it is in the vertical plane. Conversely, if the eye-movements are in the vertical plane, disappearance is more frequent when the breadth of the strip is in the vertical than when it is in the horizontal plane. An explanation will be given in Section iii, *c*. Were a periodicity grounded in the nature of the after-image process, extraneous influences, like the form and arrangement of the stimulus, ought not thus to affect the frequency of fluctuation.

(5) *The results grouped under (1), (3) and (4) can be roughly duplicated if voluntary eye-movement is brought in to cause fluctuation.* In the experiments under this heading, the same squares were used as in (1); the same strips and squares as in (3); and the same strips and arrangements with reference to the direction of greatest eye-movement as in (4). The cor-

respondence in the results of the two series of experiments is extremely high. Here, then, is a strong indication that eye-movement is a causal factor in fluctuation. Another factor, however, is required for the complete explanation of the results. The methods employed in (1) and (3) were especially devised to vary the amount of involuntary eye-movement from observation to observation. Yet their results were approximated by the introduction of voluntary eye-movement, the amount of which was kept constant from one observation to another. Obviously, therefore, a second factor is at work, which is affected by eye-movement, and which in turn acts upon the after-image. A more complete explanation is given later in terms of streaming.

(6) *An increase of the time of stimulation increases the number of fluctuations of the after-image.* The time of stimulation ranged for the different observers from 10 to 100 sec. Increase of the time of stimulation brought with it an increase in the intensity of the after-image, an increase in eye-movement (shown by the records), and an increase in the fluctuation of the after-image. It cannot, of course, be determined off-hand which of the first two variations is the cause of the third. From the evidence already at hand, it seems probable that eye-movement is responsible for the increase of fluctuation. Moreover, there is no obvious reason why a more intensive after-image, on the ground of its intensity alone, should begin to fluctuate sooner (at a greater intensity) or should fluctuate more frequentiy after it does begin,—as happens uniformly under the present conditions. The problem is, however, capable of definite experimental analysis. Increase of intensity can be obtained by a method which does not cause an increase of eye-movement; namely, by increase of the intensity of the stimulus. In this case there is no increase of the number of fluctuations. We thus have new and positive evidence that eye-movement is a causal factor in fluctuation. A long period of fixation increases involuntary eye-movement, and this again increases the frequency of fluctuation, apparently in the same proportion.

(7) *The observers most sensitive to the methods used to distribute fixation showed the widest range of variability of fluctuation and duration.* The observers ranged from very stable to very sensitive. The tables show a variation in results from individual to individual, corresponding to the differences in sensitivity to disturbances of fixation. Thus, B and A were very sensitive, W much less sensitive, and M the least sensitive of all. Correspondingly, B's and A's results are very different for the different variations; W's less, and M's still less different. This correlation, it is clear, furnishes evidence in favor of eye-move-

ment as clear-cut and decisive as that to be drawn from the changes in result produced by the different methods in the case of a single individual.

(8) *Increase of practice in fixation brought with it a decrease in the frequency of fluctuation and an increase in the duration of the after-image.* Towards the close of the semester's work it became clear from the eye-movement records that there was an increased ability to fixate on the part of all the observers. There was also a corresponding decrease in the frequency of fluctuation. The pitch of the curve of frequency for the method of areas, *e. g.*, was lessened. At both ends of the series, areas that had fluctuated earlier would not now fluctuate at all, and areas that formerly fluctuated readily now underwent fewer fluctuations. The effect of practice was especially marked in the case of M. There was also, as the work advanced, a decreased sensitivity to the methods used to disturb fixation. With practice fixation became progressively more stable. A general straightening of the frequency curves, for all the methods employed, was the result.

b. General description of method and apparatus. All methods were ruled out as ineffective that did not produce changes in result markedly greater than the variations occurring from time to time without change of experimental conditions. Experiments were planned in series to be finished at a single sitting. Results obtained at different sittings were never compared directly, nor were the results from broken or interrupted series included in the general averages. The order of presentation of the members of a series was changed from time to time, in order to rule out time and practice errors. Care was taken in the selection of *O*'s to get a random sampling of types both as to visual organization and as to experience. As little as possible was left to the uncontrolled introspection of the *O*'s. The analyses were provided for in the experimental variations, and the *O*'s were asked only for the simplest judgments, and were kept in entire ignorance of the problem and plan of experimentation.

The recording apparatus used throughout consisted of kymograph, telegraph-key and electro-magnetic recorder, with electro-magnetic time-marker regulated by a chronometer set to half-seconds. The experiments were conducted in a long optics room lighted at the one end by two windows reaching from near the floor to the ceiling. The *O*, head in rest, was seated between these windows, so that the light coming from either side and above fell upon the projection-field of engine-gray cardboard 75 cm. in front. The time of stimulation, unless otherwise stated, was 40 sec., and the unit of record was 1 sec.

c. Results in detail. The work was begun three years ago

in the Cornell University laboratory, and continued in the laboratory of the University of Arizona during the year 1905-6; in the laboratory of the University of Colorado during the fall semester of 1906-7; in the Cornell University laboratory during the spring semester of 1906-7; and finished in the laboratory of Bryn Mawr College during the fall semester of 1907-8. The results have been verified both in drill courses and in research, with a wide range of observers of diverse training and experience. The present section of the work was done for the most part in the laboratory of the University of Colorado. A part of it has been repeated in the laboratory of Bryn Mawr College. The following persons served as observers: Professor J. H. Bair (B); the Misses Alden (A), Walter (W), Montgomery (M) and Wright (Wr), students in his laboratory; and Miss Stout (S), a student in the laboratory of Bryn Mawr College.

In the tables account is taken of the following points: the number of fluctuations, the first phase of visibility, the average of the phases of visibility, the sum of the phases of visibility, the average of the phases of invisibility, the sum of the phases of invisibility, and the sum of the phases of visibility and invisibility. One less than the number of phases of visibility has been taken as the number of fluctuations. The last disappearance has not been counted in estimating the number of fluctuations. This number is of value to us as a measure of the disturbance in the after-image process. A still better measure, however, is the frequency of fluctuation, expressed by the average of the phases of visibility. The length of the first phase of visibility is of importance as indicating at what intensity the after-image begins to fluctuate. In general, the stronger the operation of disturbing factors, the greater should be the intensity at which the after-image begins to fluctuate. Accordingly, then, we should expect an increase of eye-movement to decrease the first phase of visibility, and a decrease of eye-movement to increase it.

The conventional use of the term duration has been departed from in this discussion. By duration is here meant the sum of the phases of visibility. This use of the term is in the first place strictly accurate, so far as we have any immediate presentation to consciousness of the after-image process; and, so considered, it has, in the second place, more direct bearing upon the problem of the restoration of the adapted stimulus.

No especial significance is attributed to the phases of invisibility. They are included in the tables merely that a complete account of the temporal course of the after-image may be given.

(1) *Fluctuation occurs only within a limited range of areas.*

This statement holds of all the colors used: Hering standard red, green, blue and yellow. The result varied somewhat, however, for the different colors. The red after-image showed itself throughout to be the most instable. It fluctuated most frequently, had the shortest duration, and was most affected by the various disturbances. At the other extreme was the yellow after-image. It proved the most stable of all. For this reason, since space does not permit of giving results from all the stimuli, the yellow image derived from Hering blue as stimulus has been selected for the following tables; it affords the most rigid test of the effect of eye-movement on the temporal course of the after-image in general.

Areas ranging from .5 by .5 cm. to 61 by 50 cm. (the latter being the dimensions of a single sheet of Hering paper) and viewed at a distance of 75 cm. were employed. A still larger area was required for some observers, if no fluctuation was to ensue. This increase in size was obtained by moving the stimulus nearer to the observer.

It will be noticed from the tables that, with small areas, little or no fluctuation occurs. Then there is increase up to a certain point, namely, 10-20 cm. square, when decrease begins. Fluctuation disappears entirely in the neighborhood of 60-65 cm. square.

If we ask how far eye-movement is to be regarded as a causal factor in the increased fluctuation from small to medium areas, and its consequent decrease, we find the following evidence. Over the range of areas showing increase of fluctuation the observers spoke of a strong conscious tendency to look away from the fixation point in order to see what was happening towards the margins. This tendency constituted a distracting factor for fixation. With small areas, the whole after-image lay within the field of direct observation; consequently there was nothing to distract fixation. With the next set of areas, the edges passed into the field of indirect observation, but were still noticeable. Hence they disturbed fixation in various ways. In the first place, they broke the uniformity of the field of vision; and in the second place the observer was instructed to register only total disappearances, disappearances over the whole area. The margins were not clearly visible, and so tempted the eyes to a readjustment which would bring them into the field of clear vision, and thus make observation easier. With the third set of areas, the margins had passed so far from the field of direct observation as to be of little concern to the observer. The field was of an uniform color and brightness, and the margins did not compel attention.

The eye-movement records confirm these introspections. There is increase of movement through the range of areas

TABLE II

A. Fluctuation occurs only within a limited range of areas. Results showing the effect of variation of area on the fluctuation of the after-image.

Area	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
.5x .5cm.	0	18.7	18.7	18.7	0	0	18.7
1.5x1.5 "	1	48.6	24.8	49.6	5.5	5.5	55.1
5 x 5 "	2	59.5	21.6	64.8	2.4	4.9	69.7
10 x 10 "	12	21.5	5 9	76.7	2.4	28.8	105.5
20 x 20 "	7	23.5	10.0	80.0	3.2	22.4	102.4
40 x 40 "	3	42.0	16.0	64.0	2.1	6.3	70.3
61 x 50 "	0	72.0	72.0	72.0	0	0	72.0

TABLE III. (Observer W.)

Area	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
1 x 1 cm.	2	14.7	20.2	65.5	1.6	3.3	69.8
5 x 5 "	2	18.0	27.9	83.8	1.0	2.0	85.8
10. x 10. "	3	15.5	21.1	85.0	1.0	3.1	88.7
20 x 20 "	6	21.0	11.6	81.3	3.9	11.8	93.1
40 x 40 "	5	8.8	10.5	63.2	4.3	21.45	84.6
61 x 50 "	1	35.5	31.6	63.3	2.6	2.6	65.9
61 x 50 " 55 cm. distant	1	60.0	47.7	95.5	1.0	1.0	96.5
61 x 50 cm. 35 cm. distant	0	50.5	50.5	50.5	0	0	50.5

showing an increase of fluctuations, and decrease where there is a decrease of fluctuation. Nevertheless, the explanation is not so simple as this correlation implies. The effect of eye-movement on fluctuation is not direct. Eye-movement affects the after-image only through its effect on the streaming phenomenon; and the final word of explanation must be deferred until we come to discuss that subject.

Two methods were used for the investigation of eye-move-

TABLE IV. (Observer M.)

Area	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
1.5 x 1.5 cm.	0	52.0	52.0	52.0	0	0	52.0
5 x 5 "	2	27.3	18.4	55.2	1.3	2.5	57.7
10 x 10 "	6	35.5	7.9	55.3	2.5	7.5	62.8
20 x 20 "	6	35.0	8.0	56.4	1.3	7.8	62.2
40 x 40 "	5	32.1	8.8	53.1	1.6	8.3	61.4
61 x 50 "	4	42.2	10.5	52.5	1.3	5.3	57.8
61 x 50 " 35 cm. distant	0	52.0	52.0	52.0	0	0	52.0

ment. In the first method, the shifting of the after-image from the stimulus during fixation was chosen as a measure of the eye-movements taking place. This method had the disadvantage that the eye-movement could not be recorded while fluctuation was going on. However, by using only experimental variations that produced marked changes in the steadiness of fixation, by alternating eye-movement with fluctuation records, and by taking a large number of records for each experimental device, this objection was practically obviated; more especially as only comparative results were desired. The method, too, has the very great advantage of sensitivity. When, *e. g.*, the observer is stationed 1 meter from the stimulus, a shift of the after-image 1 mm. to either side represents an eye-movement (measured by the chord of the arc) of approximately .017 mm.[1] The sensitivity of this method is directly proportional to the distance of the observer from the stimulus, and is limited only by the range of distinct vision. Under favorable conditions exceedingly slight tremors can be detected. In fact, as a gauge for small eye-movements, the method is far more sensitive than the methods of photography and of mechanical registration.

In the second method, the after-image was projected without a fixation point, and a record was made of the time during which it was moved and of the time during which it was still. This method was somewhat defective, because the range of movement (a very important factor in the causing of fluctuation) could be indicated only roughly, by the introspective

[1]Calculation is made from the average of the first and second principal focal distances of the normal eye as estimated by Listing: see Helmholtz, Physiol. Optik, 90.

reports of the observers as to whether the after-image moved rapidly or slowly. However, it proved a valuable supplement to the former method, since by it the eye-movements were registered while the fluctuations were actually going on. One could thus tell at once whether disappearance came as a direct effect of eye-movement, *i. e.*, whether it came while the eye was moving or immediately after it had moved; or whether it came in an interval of rest. The effect of vigorous, quick movements could also be compared with that of weaker and slower movements.

Two forms of this second method were employed. In the one, the eye-movement and the phases of appearance and disappearance of the after-image were both recorded; in the other, the eye-movements alone were recorded. The first form gave a direct tracing of the effect of eye-movement upon fluctuation. The recording was done as follows. The after-image was projected without a fixation point, and the key was held down as long as the after-image was in motion and released when it came to rest. When the after-image disappeared, the key was given two quick pressures, and then released until the after-image returned, when the record of movement went on as before until the next disappearance. A certain complication arose with this form of the method. The effort to record both eye-movement and fluctuation seemed to interfere with the course of the after-image, so that fluctuation occurred more frequently than when fluctuations alone were recorded. However, the change was merely a general rise in the scale of frequency. The variations from device to device stood out just as plainly as when the alternative method was used. The reason of the more frequent fluctuations is, probably, that the divided attention necessitated, or rather that the rapidly alternating direction of attention resulted in, a less steady fixation. On this account, the records that were meant to show simply the variation in eye-movement due to the various devices were taken according to the second method: eye-movement alone was recorded. Only these results will be given here, since the others cannot be adequately stated in tabular form,and space forbids the separate publication of every set. The statement must suffice that causal connection between eye-movement and fluctuation is directly evident upon the inspection of the results in question.

For the investigation of the effect of variation of area upon eye-movement, the second method (second form) was used. The after-images were projected without aid from fixation; and the key was held down as long as they were in continuous motion, and released during the intervals in which they were at rest. The following results were obtained.

TABLE V

A. Eye-movement with variation in the area of the stimulus. Showing that an increase in the area of the stimulus first increases, then decreases, the involuntary eye-movement occurring when the after-image is observed.

Area of Stimulus	Time Observed	Time Moving	Time Still	Time Moving. ÷ Time Still	Rate of Movement
.5 x .5cm.	14.5	4.5	10.0	0.45	Slow
1.5 x 1.5 "	19.5	6.4	13.1	0.48	"
5 x 5 "	28.3	10.5	17.8	0.59	"
10 x 10 "	34.0	26.45	7.55	3.50	Fairly Rapid
20 x 20 "	37.0	20.5	16.5	1.24	Moderate
40 x 40 "	65.2	24.5	50.7	0.59	Slow
61 x 50 "	58.0	19.4	38.6	0.50	"

TABLE VI. (Observer W.)

Area of Stimulus	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still	Rate of Movement
1 x 1 cm.	52.0	15.0	37.0	0.40	Slow
2 x 2 "	58.5	17.1	41.4	0.41	"
10 x 10 "	62.0	23.3	38.7	0.60	"
20 x 20 "	68.0	37.5	30.4	1.23	Fairly Rapid
40 x 40 "	87.0	46.26	40.74	1.13	"
61 x 50 "	96.2	21.3	74.9	0.28	Slow

(2) *Whatever renders fixation unsteady increases the frequency of fluctuation and decreases the duration of the after-image.* The stimulus was a square of standard Hering blue, 3 by 3 cm., fastened upon a large square of engine-gray cardboard. A square of the same cardboard was used as a background upon which to trace the course of the after-image. The effectiveness of the methods employed to disturb fixation was determined by records which showed the range and frequency of the eye-movements produced, and the total time during which the eyes were moving. The methods themselves were five in number. (*a*) The stimulus was fixated at its centre, and the after-image was projected without a fixation-point. (*b*) The stimulus was fixated at its centre, as before, and the after-image

TABLE VII. (Observer M.)

Area of Stimulus	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still	Rate of Movement
1.5x1.5 cm.	50	7.6	41.5	0.18	Slow
5. x 5. "	56	25.0	31.0	0.80	"
10 x 10 "	60	41.5	16.5	2.50	Fairly Rapid
20 x 20 "	58	40.5	19.5	2.08	"
40 x 40 "	60	39.8	20.2	1.98	Moderate
61 x 50 "	58	35.2	22.8	1.54	"
61 x 50 " (35cm. dis.)	57	21.4	35.6	0.60	Slow

was observed by help of a fixation-point. All the observers, with one exception, found the point of service for holding the after-image steady. The exception was Dr. Bair, for whom any effort at muscular control resulted in involuntary twitchings. In his case, therefore, the eye-movement records showed a greater unsteadiness of regard, when effort was made to fixate the point, than when the image was traced upon the blank surface of the cardboard. (*c*) When the after-image was projected without a fixation-point, it tended uniformly to move off in some particular direction, varying with the observer. Whatever be the explanation of this phenomenon (it may possibly be due to a faulty centering of the image upon the retina, itself the result of some maladjustment of the visual mechanism, —movement resulting as a reflex tendency to more accurate fixation), advantage may be taken of it to exaggerate or to correct eye-movement. In order to exaggerate the movement, the direction of the tendency was carefully determined at the beginning of the experimental series. The stimulus was then fixated at a point placed in the line determined by this tendency to movement, but in the *opposite* direction from the centre of the stimulus. Part or all of the stimulus was thus thrown into indirect vision, and the tendency of the after-image to move was increased,—seemingly in proportion of its displacement from the central portion of the field of vision. The reflex movement which tends to centre the after-image in the field of vision added to the natural tendency to movement; and the after-image, projected without a fixation point, moved off rapidly in the direction planned. (*d*) To correct the tendency to movement, the stimulus was fixated at a point placed in the line of the movement, but in the *same* direction from the centre of the stimulus. The consequent displace-

ment of the after-image set up a tendency to movement which counteracted the natural tendency. By careful adjustment it was possible to obtain a fair balance of the two factors, so that the after-image was held steady when projected without a fixation point. Even under the rough conditions of our experiments this adjustment proved, for some observers, the best method of controlling fixation that we could devise.[1] (*e*). For some observers, the best aid to fixation was found to be a square, drawn on the cardboard, of exactly the same size and shape as the after-image, with a point placed at its centre. When the eye moved, the after-image was observed to slip from the square frame; and the observer was thus able to correct the movement before it had attained any considerable range. With the combination of square and central point, the observer had the double advantage of the aid to fixation and the conscious check upon movement. With the point alone, there is little or no conscious control of movement; for the point has to move so far into the field of indirect observation that it is recognized as occupying a different position before the control is operative and the eye can refixate. The distraction to fixation presented by the sides of the square was probably little, because the figure was small enough to be included, practically as a whole, in the field of direct observation. At all events, it did not offset the advantage in the cases of M, A, and Wr.

For A, the after-image, projected without aid to fixation, first moved off slowly to the left, but soon turned sharply and moved much more quickly up and to the right, the latter being the stronger component in the movement. All fluctuations occurred during the second phase. Hence the drift to the left was disregarded in the methods used for correction and exaggeration. To exaggerate the movement, the fixation point was placed 9 mm. below and 12 mm. to the left of the centre of the stimulus. To correct, it was placed 9 mm. above and 12 mm. to the right of the centre.

For B, the after-image moved up and to the right. To exaggerate this movement, the fixation point was placed 12.5 mm. below the centre of the stimulus and 10.5 mm. to the left. To correct, it was placed 8.5 mm. above the centre and 8.5 mm. to the right.

For W, the after-image tended to move up and to the right.

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[1] The direction of the tendency to movement, for the different observers, will be stated in the discussion of the tables. For the students passing through the junior laboratory course, it seems most frequently upward or upward and to the right. The dominant component appears to determine the observer's type as to frequency and range of movement in the horizontal and vertical planes.

TABLE VIII

A. Whatever renders fixation unsteady increases frequency of fluctuation of the after-image and decreases its duration. Whatever aids fixation produces the opposite effect.

Variation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis.+ Invis.
Square	1	44.0	25.0	50.0	6.0	6.0	56.0
With Point	2	39.2	15.8	47.4	5.7	11.4	58.8
Without Point	3	22.0	10.2	40.8	3.9	11.7	52.5
Exaggerated	5	9.5	4.0	24.0	1.4	7.0	31.0
Corrected	2	36.8	21.5	64.5	4.5	9.0	73.5

TABLE IX. (Observer B.)

Variation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
With Point	10	17.5	3.1	34.1	1.3	13.0	47.1
Without Point	9	36.0	6.2	62.0	1.2	10.8	72.8
Exaggerated	14	26.0	1.1	16.5	1.0	14.0	30.5
Corrected	8	49.5	9.0	81.0	1.5	12.0	93.0

TABLE X. (Observer W.)

Variation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Without Point	4	13.0	15.2	76.0	.81	3.2	79.2
With Point	3	16.0	22.4	89.6	.55	1.6	91.2
Exaggerated	7	6.0	8.2	65.6	.52	3.6	69.2
Corrected	2	20.7	32.0	96.0	.50	1.0	97.0

Accordingly, to exaggerate this movement, the fixation point was placed 23 mm. below and 23 mm. to the left of the centre of the stimulus. The best correction of the movement was obtained by placing the point 12 mm. above and 6 mm. to the right of the centre. W's records showed an individual peculiarity, in that the first phase of visibility was relatively short, while the last was long. There is no obvious explanation.

TABLE XI. (Observer M.)

Variation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Without Point	5	8.7	7.0	42.0	3.7	18.5	60.5
With Point	2	24.0	24.1	72.3	.67	1.3	13.6
Exaggerated	6	2.4	4.3	30.1	1.1	6.6	36.7
Corrected	1	37.0	29.7	59.4	.9	0.9	60.3

For M, the after-image moved up and to the right. To exaggerate this movement, the fixation point was placed 12 mm. below the centre of the stimulus and 12 mm. to the left. To correct, it was placed 7 mm. above the centre of the stimulus and 7 mm. to the right.

For Wr, the after-image moved up and to the right. To exaggerate this movement, the fixation point was placed 11.5 mm. below and 11.5 mm. to the left of the centre of the stimulus. To correct, it was placed 10 mm. above and 10 mm. to

TABLE XII. (Observer Wr.)

Variation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Without Point	3	81.5	26.5	106.2	2.3	9.2	115.4
With Point	2	92.0	40.3	120.9	1.8	5.4	126.3
Exaggerated	3	45.0	17.2	68.8	1.5	6.0	74.8
Corrected	1	63.5	45.0	90.0	2.5	2.5	72.5
Square	1	71.2	42.0	84.0	2.8	2.8	86.8

TABLE XIII

W. Showing the effects of voluntary control.

Variation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Without Point, No Effort	5	7.0	10.8	64.8	1.4	7.0	71.8
Without Point, Effort	3	16.7	19.9	79.6	1.1	3.3	82.9

the right of the centre of the stimulus. Wr's records showed very long phases of invisibility.

It was found that, when the after-image was projected without a fixation point, frequency of fluctuation was considerably increased if the observer made no particular effort to hold the eyes steady. The following results illustrate this point.

This method showed, more plainly than any other, the effect of a variation in the amount of eye-movement upon the frequency of fluctuation. For this reason, the eye-movements resulting from the various devices employed were studied with some care. Three cases were made of this determination: specimen results will be given from each one.

(1) In order to compare the movements occurring, first, when a point is given for fixation, and secondly when there is no such aid, we had recourse to the shift of the after-image from a colored strip. Strips of Hering's standard yellow, 5 by 50 mm., were pasted on a background of white cardboard, with the shorter dimension in the plane in which the eye-movement was to be investigated. To determine frequency, it was then necessary simply to record the appearance of the after-image to right or left, above or below the stimulus. Separate series were taken for each plane. For the determination of range of movement, narrow strips of paper of the same brightness as the background were pasted successively 2, 4, 6, 8 . . . mm. from the stimulus, and only those movements were recorded that shifted the image up to or beyond them. While the strips were inconspicuous, so that the eye was not drawn away from the fixation point, it was not difficult to observe when the image reached or passed them. The strips were also used when frequency alone was to be determined, in order that the same experimental conditions might obtain throughout. The record was made as follows. When the objects of investigation were the frequency of eye-movement, and the total times during which the eyes maintained and lapsed from their proper fixation, the observer pressed the key as the image appeared to either side of the strip, and held it down until the image was again superposed upon the stimulus. Then the key was released, and so on. Since the eyes may be said to have been in motion for practically the whole period during which the image was not superposed upon the stimulus, the method gives a record of the total time for which the eyes were still and of the total time for which they were moving. When, again, the range of movement was investigated, the key was held down only while the image was out as far as, or beyond, the strips which served as range indicators. These records, therefore, show only the times for which the point of regard was shifted a given distance from the fixation point and for which it was not.

The following tables also inform us of the direction of greatest eye-movement,—information which we need under (4) below.

TABLE XIV

A. Eye-movement: Results showing the movement in the horizontal and vertical planes, with and without a fixation point. Time of observation, 1 min.

Arrangement	Fixation	Range of Movement	No. of Movements of Given Range	A. Time Eye Moving with Given Range	B. Time not Moving with Given Range	A ÷ B
Vertical	Without Point	Record'd all	85	48.8	11.2	4.35
"	With Point	"	75	33.2	26.8	1.23
Horizontal	Without Point	"	68	41.25	18.75	2.2
"	With Point	"	64	27.3	32.7	0.83
Vertical	Without Point	4 mm.	30	13.0	47.0	0.28
"	With Point	"	25	5.1	54.9	0.09
Horizontal	Without Point	"	13	6.55	53.45	0.01
"	With Point	"	1	0.4	59.6	0.006
Vertical	Without Point	6 mm.	24	8.6	51.4	0.16
"	With Point	"	15	4.3	54.7	0.007
Horizontal	Without Point	"	9	4.75	54.25	0.008
"	With Point	"	0	0	120.0	

TABLE XV. (Observer B; time of obs., 1 min.)

Arrange-ment	Fixation	Range of Movement	No. of Move-ments of Given Range	A. Time Eye Mov-ing with Given Range	B. Time not Mov-ing with Given Range	A ÷ B
Vertical	Without Point	Record'd all	62	32.9	27.1	1.21
"	With Point	"	80	39.1	20.9	1.87
Horizontal	Without Point	"	40	28.8	31.2	0.92
"	With Point	"	56	31.6	28.4	1.11
Vertical	Without Point	2 mm.	16	13.5	46.5	1.29
"	With Point	"	26	19.6	40.4	0.40
Horizontal	Without Point	"	14	10.4	49.6	0.20
"	With Point	"	20	13.1	46.9	0.28
Vertical	Without Point	4 mm.	6	5.2	54.8	0.095
"	With Point	"	14	7.4	52.6	0.14
Horizontal	Without Point	"	2	3.1	56.9	0.054
"	With Point	"	10	5.3	54.7	0.96
Vertical	Without Point	7 mm.	0	0	60.0	
"	With Point	"	4	2.5	57.5	0.043
Horizontal	Without Point	"	0	0	0	
"	With Point	"	2	1	59.0	0.017

TABLE XVI. (Observer W; time of obs., 1 min.)

Arrange-ment	Fixation	Range of Movement	No. of Move-ments of given Range	A. Time eye mov-ing with given Range	B. Time not mov-ing with given Range	A ÷ B
Vertical	Without Point	Record'd all	55	40.43	19.5	2.07
"	With Point	"	46	13.67	46.32	0.29
Horizontal	Without Point	"	47	23.4	36.6	0.64
"	With Point	"	35	10.2	49.8	0.20
Vertical	Without Point	2 mm.	45	19.4	40.5	0.48
"	With Point	"	32	9.7	50.25	0.19
Horizontal	Without Point	"	39	14.5	45.5	0.31
"	With Point	"	28	8.27	51.72	0.16
Vertical	Without Point	4 mm.	21	12.2	47.75	0.25
"	With Point	"	16	4.22	57.75	0.07
Horizontal	Without Point	"	16	9.7	50.3	0.19
"	With Point	"	10	3.15	56.85	0.05
Vertical	Without Point	6 mm.	5	1.9	58.1	0.0024
"	With Point	"	3	1.15	58.85	0.0019
Horizontal	Without Point	"	3	1.05	58.95	0.0018
"	With Point	"	0	0	60.0	

TABLE XVII

Arrangement	Fixation	Range of Movement	No. of Movements of Given Range	A. Time eye moving with given Range	B. Time not moving with given Range	A ÷ B
Vertical	Without Point	Record'd all	28	39.4	20.6	1.91
"	With Point	"	18	22.8	37.2	0.61
Horizontal	Without Point	"	19	30.6	29.4	1.04
"	With Point	"	15	19.5	40.5	0.48
Vertical	Without Point	9 mm.	3	1.5	58.5	0.025
"	With Point	"	2	0.7	59.3	0.011
Horizontal	Without Point	"	2	0.9	59.1	0.015
"	With Point	"	0	0	60.0	

(2) The shift of the after-image from the stimulus was used to determine the eye-movement for each one of the fixation devices. The square of Hering blue paper, 3.5 by 3.5 cm., was observed in turn without a fixation point, with a fixation point placed at its centre, with a fixation point displaced from its centre so as to exaggerate the movement, and with a fixation point so displaced as to correct the movement. Thus frequency and total time of movement were taken account of. Only one table will be given to illustrate these determinations.

TABLE XVIII

W. Showing the effect upon eye-movement of the fixation devices used in Table X. Time of obs., 1 min.

Variation	Time Moving	Time Still	Time Moving ÷ Time Still
Without Fixation Point	27.9	32.0	0.87
With Fixation Point	15.3	44.7	0.34
Exaggerated	38.2	21.8	1.75
Corrected	7.9	53.1	0.15

(3) The eye-movement for each one of the fixation devices was also determined by the second method (second form). The after-image was obtained as in the after-image experiments; and the key was held down as long as the image was moving, and released while it was at rest.

TABLE XIX

A. Showing the effect upon eye-movement of the fixation devices used in Table VIII

Variation	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still	Rate of Movement [1]
Without fixation point	94.9	82.8	12.1	6.84	Moderate
With fixation point	118.0	85.1	32.9	2.58	Movem't slow. Correction jerky
Exaggerated	62.2	58.5	3.7	15.81	Very rapid
Corrected	125.7	68.9	56.8	1.21	Very slow

TABLE XX

B. Showing the effect upon eye-movement of the fixation devices used in Table IX

Variation	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still	Rate of Movement
With fixation point	112.5	32.0	80.5	0.39	Movem't slow. Correction jerky
Without fixation point	94.1	47.3	46.8	1.01	Moderate
Exaggerated	50.0	46.6	3.4	13.70	Very rapid
Corrected	159.7	14.7	145.0	0.10	Very slow

(3) *The form of the stimulus affects the frequency of fluctuation.* The stimulus was, as in the former experiments, of standard Hering blue. When squares were used, they were made so small that their edges lay within the field of direct observation; they could thus exert no influence to increase eye-movement, and we should expect a minimal disturbance of the after-image. The strips, on the other hand, were made

[1] The introspections as to rate of movement have not been incorporated in the other sets of eye-movement tables. In general, when the ratio 'time moving ÷ time still' is increased, the rate of movement is also increased.

TABLE XXI

M. Showing the effect upon eye-movement of the fixation devices used in Table XI

Variation	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still	Rate of Movement
Without fixation point	84.6	35.1	49.5	0.70	Moderate
With fixation point	100.4	8.3	92.1	0.09	Movem't slow. Correction jerky
Exaggerated	70.2	50.0	20.2	2.47	Very rapid
Corrected	139.0	0	139.0		Very slow

narrow, so that, as their areas were equal to those of the squares, their ends were thrown into the field of indirect observation, and the tendency was towards increased eye-movement. Thus maximal disturbance of fixation was obtained for the given area, and correspondingly a maximal disturbance of the after-image was expected.

To illustrate: a strip .5 x .5 cm. had, as its equivalent area, a square of 1.5 cm.; a strip .5 x 10 cm., a square of 2.2 cm.; a strip .5 x 20 cm., a square of 3.1 cm.; and a strip .5 x 40 cm., a square of 4.4 cm. Only the squares of 2.2, 3.1 and 4.4 cm. fluctuated at all, while the strips showed a rapid increase in fluctuation until .5 by 40 cm. was reached, when a slight decrease occurred. A strip .5 x 20 cm., *e. g.*, gave for A 8 fluctuations, with an average phase of visibility of 7.3 sec.; while its equivalent square gave no fluctuations at all (av. vis., 71.5 sec.). No record was taken of fluctuation in parts; only total disappearances were registered. Thus the actual disturbance suffered by the strip-image was taken account of only in part.

It may be deduced from the following tables that the shape of the curve of frequency obtained by increasing the length of the strips is somewhat different from that obtained by increasing the area of the squares (Tables II-IV). If a curve were plotted by laying off the lengths of the strips along the abscissa and the frequency of fluctuation along the ordinate, the curve would start on or near the abscissa, rise fairly steeply until a length of 20-40 cm. was reached, and then bend downward slightly. It would not reach the abscissa, since with the lengths of strip used fluctuation did not cease as it did with increase of area when squares were used. The reason of this difference between the results of the two sets of experiments will be given later, in our discussion of the streaming phenomenon.

TABLE XXII

A. Form of stimulus affects frequency of fluctuation of after-image. Results showing that fluctuation is more frequent when stimulus is in form of strip, than when it is in form of a square of equivalent area

Form	Area	No. of Fluctuations	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Strip	.5 x 2 cm.	0	42.0	42.0	0	42	0	41.5
Strip	.5 x 5 cm.	2	26.0	14.0	1.0	43	2.0	44.5
Square	1.5 x 1.5 cm.	0	52.0	52.0	0	52	0	51.5
Strip	.5 x 10 cm.	6	35.0	8.0	1.2	56	7.3	63.4
Square	2.2 x 2.2 cm.	0	61.0	61.0	0	61	0	61.0
Strip	.5 x 20 cm.	8	26.5	7.3	1.6	66	13.4	79.8
Square	3.1 x 3.1 cm.	0	72.0	72.	0	73	0	71.5
Strip	.5 x 40 cm.	7	26.0	9.5	1.3	76	9.4	85.4
Square	4.4 x 4.4 cm.	3	59.5	19.0	2.0	73	6.0	79.0

TABLE XXIII. (Observer W.)

Form	Area	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
Square	.5x .5cm.	0	37.2	37.2	37.2	0	0	37.2
Strip	.5x 5 "	2	14.7	15.7	47.2	2.0	4.0	51.2
Square	1.5x1.5 "	1	20.5	33.2	64.5	1.0	1.0	65.5
Strip	.5x 10 "	3	11.5	13.2	55.0	1.4	4.2	59.2
Square	2.2x2.2 "	1	43.0	34.0	68.0	2.5	2.5	70.5
Strip	.5x20 "	3	9.7	15.7	62.8	0.9	2.7	65.5
Square	3.1x3.1 "	2	29.5	23.15	69.5	1.5	3.0	72.5
Strip	.5x40 "	3	11.5	19.2	77.0	2.9	4.4	81.4
Square	4x4 "	2	23.0	24.5	73.5	1.3	3.9	77.4
Strip	.5x61 "	3	6.0	21.8	87.5	3.6	5.5	93.0

TABLE XXIV. (Observer M.)

Form	Area	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Square	.5x .5 cm.	0	55.0	55.0	55.0	0	0	55.0
Strip	.5x 5 "	1	59.7	39.6	79.2	1.5	1.5	80.7
Square	1.5x1.5 "	0	66.4	66.4	66.4	0	0	66.4
Strip	.5x10 "	4	32.2	12.5	62.3	2.1	8.4	70.7
Square	2.2x2.2 "	1	58.5	33.0	66.0	4.0	4.0	70.0
Strip	.5x20 "	6	9.2	8.7	61.0	3.2	19.2	80.4
Square	3.1x3.1 "	1	97.0	50.5	101.0	1.0	1.0	101.0
Strip	.5x40 "	6	12.5	7.7	54.5	4.4	26.4	80.9
Square	4.4x4.4 "	2	93.0	32.5	97.5	7.1	14.2	111.7
Strip	1.5x50 "	4	20.5	10.2	51.0	3.9	15.6	66.6

The second method (second form) was here used for investigating eye-movement. The squares and strips were projected on a sheet of engine-gray cardboard, without a fixation point, and the times were recorded during which they were moving and at rest.

TABLE XXV

A. Eye-movement, with variation in form of stimulus. Showing that more involuntary eye-movement occurs during observation of after-image when stimulus is a strip than when it is a square of equivalent area.

Form of Stimulus	Dimensions of Stimulus	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still
Strip	.5 x 5 cm.	24.12	12.02	12.10	0.98
Square	1.5 x 1.5 cm.	35.45	9.40	26.05	0.36
Strip	.5 x 10 cm.	30.00	15.25	14.75	1.03
Square	2.2 x 2.2 cm.	46.80	16.35	30.50	0.53
Strip	.5 x 20 cm.	36.55	24.53	12.03	2.04
Square	3.1 x 3.1 cm.	50.07	19.98	30.72	0.65
Strip	.5 x 40 cm.	38.85	19.50	19.35	1.01
Square	4.4 x 4.4 cm.	55.80	25.50	30.30	0.84

TABLE XXVI. (Observer M.)

Form of Stimulus	Dimensions of Stimulus	Time Observed	Time Moving	Time Still	Time Mov'g ÷ Time Still
Strip	.5x 5 cm.	36.54	14.92	21.62	0.69
Square	1.5x1.5 "	46.90	8.80	38.10	0.23
Strip	.5x10 "	43.25	21.07	22.18	0.95
Square	2.2x2.2 "	49.20	13.30	35.90	0.37
Strip	.5x20 "	49.70	31.23	18.47	1.69
Square	3.1x3.1 "	52.30	19.20	33.10	0.58
Strip	.5x40 "	54.80	35.50	19.30	1.84
Square	4.4x4.4 "	57.90	24.70	33.20	0.74

TABLE XXVII. (Observer W.)

Form of Stimulus	Dimensions of Stimulus	Time Observed	Time Moving	Time Still	Time Mov'g ÷ Time Still
Square	.5x .5 cm.	63.0	15.35	47.65	0.32
Strip	.5x .5 "	64.5	26.20	38.80	0.68
Square	1.5x1.5 "	82.0	21.55	60.45	0.35
Strip	.5x10 "	73.5	33.90	39.60	0.86
Square	2.2x2.2 "	89.0	32.30	56.70	0.57
Strip	.5x20 "	82.0	38.90	42.10	0.90
Square	3.1x3.1 "	89.75	33.50	56.25	0.60
Strip	.5x40 "	97.5	48.25	49.25	0.98
Square	4.4x4.4 "	82.0	35.40	46.60	0.76

(4) *The arrangement of the stimulus with reference to the direction of greatest eye-movement affects the frequency of fluctuation and the duration of the after-image.* The stimuli for A were strips of Hering standard blue, .5 cm. wide and of various lengths; the stimuli for W were strips of 2 cm. wide. These were placed first in the vertical and then in the horizontal plane, and fixated at the centre for 40 sec. The images were observed on a background of engine-gray cardboard, with a fixation point. The tables show more frequent fluctuation, a shorter first phase of visibility, and a shorter total visibility, when the length of the strips is in the vertical plane. Correspondingly, the eye-movement tables show a greater range and frequency in the direction of the lesser dimension of the after-image (the horizontal plane). The results found with the

trained observers have been paralleled in laboratory practice. Those published from this latter class were obtained with Miss Stout (S), a student at Bryn Mawr College.

TABLE XXIX

A. The arrangement of stimulus with reference to direction of greatest eye-movement affects frequency of fluctuation and duration of after-image.

Arrangement of Strip	Dimensions of Strip	No. of Fluctuations	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Vertical	.5 x 5 cm.	2	11.1	33.2	0.5	1.0	34.3
Horizontal	" "	0	47.5	47.5	0	0	47.5
Vertical	.5 x 10 cm.	5	4.0	24.0	0.9	4.5	29.5
Horizontal	" "	1	24.1	48.2	2.9	2.9	51.1
Vertical	.5 x 20 cm.	7	3.8	30.4	1.1	7.7	61.5
Horizontal	" "	1	29.3	58.6	2.9	2.9	38.1
Vertical	.5 x 40 cm.	6	8.7	60.9	0.8	4.8	65.7
Horizontal	" "	3	19.1	76.4	0.7	2.1	78.5

TABLE XXX. (Observer W.)

Arrangement	Area	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
Vertical	2x5 cm.	2	13.0	16.4	49.2	1.5	3.0	52.2
Horizontal	2x5 "	2	16.5	21.8	65.4	1.6	3.2	68.6
Vertical	2x10 "	2	22.0	17.3	52.0	1.5	3.0	55.0
Horizontal	2x10 "	2	34.5	22.1	66.5	1.4	2.8	69.3
Vertical	2x20 "	3	24.5	16.6	66.5	2.0	6.0	72.5
Horizontal	2x20 "	3	37.0	21.8	87.2	1.3	3.9	91.1
Vertical	2x40 "	4	22.0	16.6	84.0	1.6	6.4	90.4
Horizontal	2x40 "	3	27.6	22.0	88.0	1.4	4.2	92.2
Vertical	2x50 "	3	12.0	16.7	66.8	0.9	3.6	70.4
Horizontal	2x50 "	3	19.5	20.5	82.0	0.7	2.8	84.8

The eye-movement records of A and W, in the horizontal and vertical planes, are given in Tables XIV, XVI. In both cases, for every point recorded, there was marked excess in the horizontal plane. Owing to lack of time this determination was not made for S. The fact, however, that in S's duplication series the eye-movement across the strip was always more

TABLE XXXI. (Observer S.)

Arrangement of Strip	Dimensions of Strip	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
Vertical	.5x 5 cm.	2	14.2	9.6	28.8	2.6	5.2	34.0
Horizontal	.5x 5 "	1	32.0	17.4	34.8	4.0	4.0	38.8
Vertical	.5x10 "	3	12.6	7.5	30.0	2.1	6.3	36.3
Horizontal	.5x10 "	2	28.9	13.6	40.8	3.5	10.5	51.3
Vertical	.5x20 "	8	10.4	5.6	50.4	1.5	12.0	62.4
Horizontal	.5x20 "	4	27.2	13.3	65.5	2.4	9.6	75.1
Vertical	.5x40 "	5	24.5	9.8	58.8	4.0	20.0	78.8
Horizontal	.5x40 "	3	30.4	16.4	65.6	4.6	13.8	79.4

effective for fluctuation than that along it indicates that the greater frequency of fluctuation when the strip was arranged vertically was due to an excess of eye-movement in the horizontal plane.

(5) *The results in (1), (3) and (4) can be roughly duplicated by using voluntary eye-movement to cause the disappearances.* The voluntary eye-movement was regulated throughout in the following manner. The after-image was observed with the aid of a fixation point. A second point was placed 12 cm. to the right of this. At a signal, given every 3 sec. by the experimenter, the observer moved his eyes quickly out to this point and back again. He was told to record as a 'disappearance' only a case in which the after-image failed to reappear after the eyes had regained their normal fixation. Thus nothing but genuine disappearances were taken account of. Possible visual synæsthesia attending eye-movement, distraction, etc., were guarded against by the directions under which the observer worked. The after-images were blotted out as completely as after-images ever are in the case of natural fluctuation. There is not a shadow of doubt on this point. The more uniform side of engine-gray cardboard was used as background for both stimulus and after-image. Hence there was no danger of disturbance by possible distractions due to movement of the eye over an irregularly marked surface.

i. Fluctuation occurs only within a limited range of areas. Just as when the observation is made under the conditions of ordinary fixation, the after-effect of general adaptation does not fluctuate under the influence of voluntary eye-movement. Nor do after-images fluctuate beyond a comparatively limited range of areas. Within this range the results are very similar to those obtained in the case of natural fluctuation. Large images do not fluctuate at all; small images little, if at all;

while middle-sized images alone fluctuate readily. The curve of frequency takes the same general shape as it does with natural fluctuation.

TABLE XXXII

A. Duplication of results by voluntary eye-movement. Method of variation of area.

Area	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
.5x .5 cm.	0	35.5	35.5	35.5	0	0	35.5
1.5x1.5 "	1	54.5	30.0	60.0	0.6	0.6	60.6
5x5 "	3	59.0	18.2	72.8	1.5	4.5	77.3
10x10 "	13	23.2	6.5	91.0	0.7	9.1	100.1
20x20 "	12	32.0	6.2	80.6	0.6	7.2	87.8
40x40 "	2	78.5	30.5	91.5	0.9	1.8	93.3
61x50 "	0	89.7	89.7	89.7	0	0	89.7

TABLE XXXIII. (Observer W.)

Area	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
.5 x .5 cm.	0	33.0	33.0	33.0	0	0	33.0
1 x 1 "	3	26.2	10.0	40.2	0.55	1.7	41.9
5 x 5 "	7	11.5	5.2	43.3	0.75	5.3	48.6
10 x 10 "	14	19.4	5.0	75.9	0.6	7.9	83.8
20 x 20 "	13	20.2	3.7	52.0	1.6	21.1	73.1
40 x 40 "	0	82.0	82.0	82.0			82.0
61 x 50 "	0	69.5	69.5	69.5			69.5

TABLE XXXIV. (Observer M.)

Area	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
.5x .5 cm.	0	47.0	47.0	47.0	0	0	47.0
1.5x1.5 "	0	71.0	71.0	71.0	0	0	71.0
10x10 "	11	54.5	6.5	82.5	2.0	22.8	105.3
20x20 "	11	31.5	5.2	63.1	1.5	17.1	80.2
40x40 "	10	37.0	5.4	60.0	1.3	13.6	73.6
61x50 "	0	46.0	46.0	46.0	0	0	46.0

ii. The form of the stimulus affects the frequency of fluctuation. The same set of stimuli were used as for natural fluctuation, and all the other conditions of the experiment were kept as nearly as possible the same. It will be observed that here, as before, the squares fluctuated little, if at all, while the strips increase in frequency of fluctuation with increase of length until a certain point is reached, when a slight decrease takes place.

TABLE XXXV

A. Duplication of results by voluntary eye-movement. Results showing that fluctuation is more frequent when stimulus is a strip than when it is a square of equivalent area.

Form	Area.	No. of Fluc-tuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
Strip	.5 x 2 cm.	0	38.5	38.5	38.5	0	0	38.5
Strip	.5 x 5 "	3	6.2	12.5	50.0	4.0	1.2	51.2
Square	1.5 x 1.5 "	0	49.0	49.0	49.0	0	0	49.0
Strip	.5 x 10 "	4	3.3	8.5	42.5	0.3	1.2	43.7
Square	2.2 x 2.2 "	0	56.0	56.0	56.0	0	0	56.0
Strip	.5 x 20 "	7	2.5	7.6	60.8	0.35	2.2	63.0
Square	3.1 x 3.1 "	1	67.0	35.8	71.6	1.9	1.9	73.5
Strip	.5 x 40 "	5	4.5	11.1	66.6	0.39	1.9	68.5
Square	4.4 x 4.4 "	2	66.0	24.5	73.5	1.1	2.2	75.7

TABLE XXXVI. (Observer W.)

Form	Area	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Square	.5x .5 cm.	0	40.5	40.5	40.5	0	0	40.5
Strip	.5x5 "	3	23.5	18.5	74.0	1.2	3.6	77.6
Square	1.5x1.5 "	1	38.5	31.3	62.6	1	1	63.6
Strip	.5x10 "	4	14.0	13.7	68.5	1.9	5.7	74.2
Square	2.2x2.2 "	1	32.6	28.2	56.4	1.5	3	59.4
Strip	.5x20 "	4	16.9	14.0	70.0	1.5	6.0	76.0
Square	3.1x3.1 "	2	29.4	26.3	78.9	1.2	2.4	81.3
Strip	.5x40 "	3	23.0	21.1	84.4	1.8	5.4	89.3
Square	4.4x4.4 "	3	26.2	23	92	1.3	3.9	95.9

TABLE XXXVII. (Observer M.)

Form	Area	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Square	.5x .5 cm.	0	48.3	48.3	68.3	0	0	68.0
Strip	.5x5 "	5	56.0	12.6	75.6	1.0	5.0	80.6
Square	1.5x1.5 "	0	68.2	68.2	68.2	0	0	68.2
Strip	.5x10 "	9	36.6	6.7	67.0	1.2	10.8	77.8
Square	2.2x2.2 "	2	55.6	23.3	69.9	2.0	4.0	73.9
Strip	.5x20 "	11	11.4	5.4	64.8	1.1	12.1	76.9
Square	3.1x3.1 "	3	82.6	26.0	104.0	2.4	7.2	111.2
Strip	.5x40 "	11	11.9	5.2	62.4	1.2	13.2	75.6
Square	4.4x4.4 "	4	86.6	19.6	98.0	2.4	9.6	107.6

iii. The arrangement of the stimulus with reference to the direction of greatest eye-movement influences the frequency of fluctuation and the duration of the after-image. Again, the same set of stimuli were used as for natural fluctuation, and the other conditions of the experiment were kept the same. For W the eye-movement was given in the horizontal plane with both arrangements of the stimuli. The tables show that when the strip was arranged with its length in the vertical plane, so that the movement was directed along its shorter dimension, the fluctuations were more frequent and the duration was shorter.

TABLE XXXVIII

W. Duplication of results by voluntary eye-movement. Showing that the arrangement of the stimulus with reference to the direction of greatest eye-movement affects the frequency of fluctuation and the duration of the after-image.

Arrange-ment	Area	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
Vertical	2 x 5 cm.	2	12.9	11.1	33.3	0.7	1.4	34.7
Horizontal	2 x 5 "	2	17.4	16.6	49.8	1.1	2.2	52.0
Vertical	2 x 10 "	3	22.8	10.2	40.8	0.8	2.4	43.2
Horizontal	2 x 10 "	3	33.6	15.4	61.6	1.0	3.0	64.6
Vertical	2 x 20 "	4	25.5	8.9	44.5	0.7	2.8	47.3
Horizontal	2 x 20 "	4	39.0	13.2	66.0	0.9	3.6	69.6
Vertical	2 x 40 "	5	22.4	9.2	55.2	0.8	4.0	59.2
Horizontal	2 x 40 "	5	38.6	15.0	90.0	1.2	6.0	9.6
Vertical	2 x 50 "	5	18.2	8.7	52.2	1.1	5.5	57.7
Horizontal	2 x 50 "	4	26.3	14.8	74.0	1.3	5.2	79.2

The law that eye-movement, when directed along the lesser dimension of the after-image, is more effective to produce fluctuation and to shorten duration was given a still more thorough verification in the cases of A and S. The strip was

TABLE XXXIX. (Observer A.)

Arrange-ment of Strip	Dimensions of Strip	Direction of Movement	No. of Fluct-uat's	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
Horizontal	.5x10 cm.	Vertical	4	3.3	16.5	0.8	3.2	19.7
"	"	Horizontal	3	14.8	59.2	0.9	2.7	61.9
Vertical	"	Horizontal	5	5.8	34.8	0.5	2.5	37.3
"	"	Vertical	1	22.0	44.0	1.4	1.4	45.4
Horizontal	.5x20 cm.	Vertical	8	5.1	45.9	0.6	4.8	50.7
"	"	Horizontal	1	25.0	50.0	0.5	0.5	50.5
Vertical	"	Horizontal	6	3.2	22.4	0.6	3.6	26.0
"	"	Vertical	2	16.4	49.2	1.1	2.2	51.4
Horizontal	.5x40 cm.	Vertical	3	13.2	52.8	0.4	1.2	54.0
"	"	Horizontal	1	34.7	69.4	0.3	0.3	69.7
Vertical	"	Horizontal	6	4.0	28.0	0.4	2.4	30.4
"	"	Vertical	4	10.1	50.5	0.5	2.0	52.5

placed with its length in the horizontal plane, and eye-movement prescribed first in the vertical and then in the horizontal plane. Then the strip was placed with its length in the vertical plane, and movement prescribed first in the horizontal and then in the vertical plane. It is thus shown that the law is not dependent upon the plane in which the strip is arranged, or the direction of the eye-movement, but that the only essential condition is that the movement be along the lesser dimension of the after-image.

TABLE XL. (Observer S.)

Arrangement of Strip	Dimensions of Strip	Direction of Movement	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
Vertical	.5 x 10cm.	Horizontal	5	21	5.9	35.4	2.6	13.0	48.4
"	.5 x 10 "	Vertical	2	30	14.3	42.9	3.2	6.4	49.3
Horizontal	.5 x 10 "	"	4	34	9.0	45.0	2.0	8.0	53.0
"	.5 x 10 "	Horizontal	3	42	13.7	54.8	3.3	9.9	64.7
Vertical	.5 x 20 "	"	16	19	4.1	69.7	2.0	32.0	101.7
"	.5 x 20 "	Vertical	9	43	7.9	79.0	2.7	24.3	103.3
Horizontal	.5 x 20 "	"	8	35	7.3	65.7	1.7	13.6	79.3
"	.5 x 20 "	Horizontal	6	58	11.5	80.5	2.7	16.2	96.7
Vertical	.5 x 40 "	"	8	45	8.7	78.3	2.6	20.8	99.1
"	.5 x 40 "	Vertical	5	56	14.1	84.6	3.1	15.5	100.1
Horizontal	.5 x 40 "	"	6	63	13.3	93.1	2.0	12.0	105.1
"	.5 x 40 "	Horizontal	4	78	20.0	100.0	2.3	9.2	109.2

(6) *An increase in the time of stimulation increases the number of fluctuations of the after-image.* Hering standard blue, 10 by 10 cm., was used as stimulus for A and M; the same

TABLE XLI

A. Increase of time of stimulation increases frequency of fluctuation of after-image.

Time of Stimulation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
10 sec.	1	42.0	27.5	55	5.4	5.4	60.4
40 "	4	78.4	23.8	119	3.1	12.4	131.4
70 "	7	97.6	20.2	162	2.4	16.8	178.8
90 "	8	94.3	17.4	157	2.1	16.8	173.8

blue, 5 by 5 cm., was used for W. The stimulus was placed on a square of engine-gray cardboard, and fixated at its centre. The after-image was projected upon a similar cardboard, with a fixation point.

It will be noticed that, with a stimulation of 10 sec., the after-image began fluctuating 8.4 sec. before its final disappearance; with 40 sec., 53 sec. before; with 70 sec., 81.2 sec. before; and with 90 sec., 87.5 sec. before disappearance. Increase in the time of stimulation results, then, in the after-image beginning to fluctuate at a greater intensity. If this result is taken in connection with the proof of an increase of eye-movement for the longer times of stimulation, it affords a strong indication that eye-movement causes fluctuation. The conclusion is made almost positive by the fact that increase of intensity, without increase of time of stimulatiom, does not increase fluctuation.

TABLE XLII. (Observer W.)

Time of Stimulation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. + Invis.
10 sec.	0	23.9	23.9	23.9	0	0	23.9
40 "	2	13.3	19.3	38.7	1.7	3.4	42.1
70 "	3	22.1	16.8	64.4	1.7	5.1	69.5

The table shows that with a stimulation of 10 sec. the after-image did not fluctuate at all; with 40 sec. it began to fluctuate 28.8 sec., and with 70 sec., 47.4 sec. before final disappearance.

TABLE XLIII. (Observer M.)

Time of Stimulation	No. of Fluctuat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
10 sec.	0	18.0	18.0	18.0	0	0	18.0
40 "	4	42.0	10.7	53.5	3.0	12.0	65.5
100 "	6	48.0	9.8	68.8	3.2	19.0	87.8

An inspection of the table will show that with a stimulation of 10 sec., the after-image did not fluctuate at all; with 40 sec., it began to fluctuate 23.5 sec., and with 100 sec. 39.8 sec. before it finally disappeared.

That increase in eye-movement follows increase in the time of stimulation was proved by the second method for the investi-

gation of eye-movement: *i. e.*, by a direct record of the movement of the projected after-image. The following results were obtained.

TABLE XLIV

A. Eye-movement with variation in time of stimulation. Showing that increase in time of stimulation increases the involuntary eye-movement occurring when after-image is observed.

Time of Stimulation	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still
10 sec.	14.7	4.95	9.75	0.5
70 "	54.5	26.4	28.1	0.94
100 "	41.0	23.0	18.0	1.28
130 "	52.2	30.4	22.3	1.36
180 "	46.0	24.4	21.65	1.12

TABLE XLV. (Observer W.)

Time of Stimulation	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still
10 sec.	64.35	22.75	41.6	0.54
40 "	85.0	29.0	46.0	0.85
70 "	96.5	48.35	48.2	1.0
130 "	102.0	55.3	46.7	1.16
190 "	95.0	59.85	35.15	1.7

TABLE XLVI. (Observer M.)

Time of Stimulation	Time Observed	Time Moving	Time Still	Time Moving ÷ Time Still
10 sec.	22.5	3.25	19.25	0.11
40 "	61.0	25.75	35.25	0.73
70 "	52.4	29.65	22.75	1.30

It was stated under the heading 'Results: General' that increase in the time of stimulation gave three results: it increased

the intensity of the after-image, the amount of involuntary eye-movement, and both the number of fluctuations and the intensity at which fluctuation began. In order to determine positively which of the two first is the cause of the third, it was necessary to study the effect of increase of intensity in isolation, *i. e.*, to increase intensity without increase of involuntary eye-movement. Since increase in involuntary eye-movement is found to follow increase in time of stimulation, the increased intensity must be secured without increase of stimulation time. This was done as follows: Hering-gray, no. 31, was used as stimulus background. Squares 5 by 5 cm. of Hering grays 15 and 7, and of Hering white, were used in turn as stimuli. The intensity of the stimulus in this case is measured by its difference from the background. Thus the intensities, roughly at least, stood in the relation 16 : 24 : 31. A square of Hering gray no. 15 was used as the background upon which to project the after-images. This shade of gray was selected because it corresponded approximately to the after-effect of the stimulus background. Thus the projection background was kept constant until the after-image, whose fluctuations were being observed, finally disappeared. This precaution may not have been necessary, but it seemed well to plan the experiment as carefully as possible. The time of stimulation was 40 sec. throughout. All these conditions were the same for the different stimuli.

A typical set of averages has been selected for publication. It will be observed that increase of intensity, without increase in time of stimulation, does not increase either the number of fluctuations, or the intensity at which fluctuation begins. Hence these results, when obtained with increase in time of stimulation, must be due to increase of eye-movement.

TABLE XLVII

A. Results showing that increase in intensity does not increase fluctuation of after-image.

Stimulus	No. of Fluct-uat's	1st Vis.	Av. Vis.	Total Vis.	Av. Invis.	Total Invis.	Vis. ÷ Invis.
Hering Gray No. 15 on " " " 31	5	20.3	7.4	44.4	2.7	13.5	57.9
Hering Gray No. 7 on " " " 31	5	22.0	9.3	55.8	1.9	9.5	65.3
Hering White on " Gray No. 31	3	28.0	15.6	62.4	2.3	6.9	69.3

(7) *The observers most sensitive to the methods used to disturb fixation show the widest range of variability in fluctuation and duration.* This will be seen by a comparison of the eye-movement with the after-image tables for each of the observers and for the various methods used. Dr. Bair (B) and Miss Alden (A) were the most sensitive; the Misses Montgomery (M) and Wright (Wr) the least; and Miss Walter (W) was of intermediate sensitivity.

(8) *Increase of practice in fixation brought with it a decrease in the frequency of fluctuation and an increase in the duration of the after-image.* Space does not permit us to show in detail this falling off in sensitivity of the different observers as the work progressed. It will be sufficient to say that it was quite marked.

iii. *How does eye-movement cause the fluctuation and shorten the duration of the after-image?*

a. It is evident that neither Fechner's nor Helmholtz' theory is adequate to the results given in the preceding Sections. Changes in illumination (Helmholtz) do not account for the shape of the curve of frequency for variation of area. Nor do they explain the fluctuation of the after-image in parts, or the effect produced upon fluctuation by variations in the form and arrangement of the stimulus. Fechner's theory, that eye-movement arouses vascular and nervous disturbances which in turn react upon the after-image, is, first of all, too indefinite. We are not told how these disturbances work, and no tangible evidence is adduced that eye-movement produces them. In the second place, even if the disturbances are granted, it is difficult to understand why they take place in this and that part of the retina while the remainder is not affected (fluctuation in parts); why they are effective in the case of certain areas, and not at all in that of others (effect of variation of area on fluctuation); and, still more, why the form of the stimulus, and its arrangement with regard to the direction of greatest range and frequency of eye-movement, etc., affect the fluctuation and duration of the after-image as powerfully as they are found to do.

Fick and Gürber follow a different course. They study the relief of adaptation not, like Helmholtz and Fechner, from the side of the negative after-image, but from the positive side. They show in various ways that the color or brightness of a stimulus to which the eye has been adapted is restored by eye-movement. They contend that adaptation is a phenomenon of fatigue, and that eye-movement relieves it, chiefly, by facilitating the removal of the fatigue products from the retina; less importantly, by increasing the delivery of new material to the

fatigued end-organs. This hypothesis, though perhaps the most promising of all the eye-movement theories, is at the same time scarcely less speculative than the others. The passage of lymph to and from the retinal elements is a necessary postulate of metabolism; but Fick and Gürber give no direct or positive proof that eye-movement facilitates the exchange; nor has the proof been brought by any subsequent investigator.

Vascular disturbances in the retina are alleged as *indirect* evidence. The following authorities may be cited upon this point. On the negative side, we find A. Coccius (Ueber die Anwendung des Augenspiegels, etc., 1853, 20), who was the first to investigate the matter, asserting that the disturbances are not present in the case of quick, short eye-movements. O. Becker (Archiv f. Ophthalmol., XVIII, 1, 1872, 242) contends that eye-movement exerts no especial influence, since he finds fluctuations in the caliber of the retinal vessels when the eye-muscles are paralyzed by atropine. On the positive side, A. v. Graefe (Archiv f. Opthalmol., I, 2, 1855, 387) establishes the general principle that eye-movement causes an increase of pressure in the vitreous humor; hence every change of fixation is followed by an increase of vascular pulsation in the retina. Michell (Lehrbuch der Augenheilkunde, 1. Aufl., 547) observed that eye-movement causes a paling of the retinal capillaries. W. Dobrowolsky (Centralblatt f. d. medic. Wissensch., 1870, 20 and 21), working on a dog, observed frequent changes in the calibre of the retinal capillaries. These changes disappeared, however, when motor paralysis was produced by curare; the capillaries also became paler when the eye-muscles were electrically stimulated. Fick and Gürber themselves, working both on the human eye and on the eyes of dogs, were able in some cases to observe the effect of eye-movement on the calibre of the retinal vessels. They found, *e. g.*, in the case of the human eye, that when the eyes are held steadily upon some distant object, vascular changes are not noticeable, but that noticeable changes occur when the eyes are moved. They believe that these changes are not normal pulsations due to the heart's action, but are directly caused by the eye-movements. The natural pulse, they say, is not observable for various reasons: thus, it may possibly be obscured by the rapidity of the heart's action. This supposition seems to receive confirmation from the results of experiments on dogs. When the dogs were put under the influence of chloral or morphine, and all the eye-muscles severed, a rhythmical change in the calibre of the retinal vessels was plainly noticeable. Since eye-movement could not operate to produce this rhythm, they regard it as that of the natural retinal pulse, now rendered observable through the slowing of the heart's action by the drugs used. On the whole, however, and having regard both to their own work and to that of others, Fick and Gürber did not consider the evidence that eye-movement influences retinal circulation to be entirely satisfactory.

Moreover, if we look at the problem from the side of the negative after-image, the hypothesis can apparently be of service only in explaining the effect of eye-movement upon the total duration of the after-image. Facilitation of the removal of fatigue products does not account for fluctuation; for if the fatigue material is carried away so completely as to cause the disappearance of the after-image, there is no satisfactory or plausible reason why it should accumulate again, and cause the

after-image to reappear, when the eye has in the meantime undergone no additional stimulation.[1] Since, then, the hypothesis cannot explain a simple case of fluctuation, it manifestly cannot account for the variations in the phenomenon discussed in the foregoing pages;[2] and, failing in this regard, it manifestly has not taken into consideration all the factors that operate to relieve adaptation.

b. Much to our surprise, the solution of the problem came with the observation of a phenomenon which, for want of a better term, will be called the 'streaming phenomenon.' We turn to its consideration with the reluctance that a writer must feel in pointing out a new phenomenon in a field so old and so minutely canvassed as that of vision, but nevertheless with a full sense of responsibility. The phenomenon was first observed in 1905, and since that time has been carefully investigated by the writer with the aid of nine observers: seven students of psychology and two laymen. All were sceptical at the outset; but later, independently of one anothers' and of the writer's observations, were able to describe the phenomenon in detail, and to sketch the more prominent of the multitude of stream patterns.

1. A brief description is difficult. When one sits with lightly closed lids, which must be kept from quivering, before a bright diffuse light, such as that of a partly clouded sky,

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[1] Fick and Gürber do not treat their problem from the side of the negative after-image, nor are they primarily concerned with this aspect of adaptation (especially with the fluctuation of the negative after-image); at the same time, they offer an explanation of this fluctuation. The explanation, however, does not account for the various cases and types of fluctuation; and its basis is both hypothetical and, in terms of the results of our own observers, contrary to fact. It runs as follows: "Augenbewegungen und Accommodation quetschen die Netzhaut gleichsam aus, das negative Nachbild verschwindet. Aber nicht aus der ganzen Netzhaut werden die Stoffwechselproducte entfernt, sondern nur aus der empfindlichsten Schichte; darum taucht das negative Nachbild in demselben Masse wieder auf, in dem sich die Stoffwechselproducte wieder über die empfindlichste Netzhautschichte verbreiten" (Archiv f. Ophthalmol., XXXVI, 300). Observation shows rather that instead of the waste material being forced from the after-image area by eye-movement, and returning to it when the intraocular pressure is relieved, just the reverse movement of material takes place. That is, eye-movement causes a wash of material from some part of the surrounding retina over the after-image area. As long as this streaming material is passing over the area, the after-image cannot be seen. When it has passed beyond it, the image reappears.

[2] It is not clear, *e. g.*, why the waste material forced from the after-image area does not return, to cause the reappearance of large and small after-images, as it does so readily in the case of after-images of medium area. Similar difficulties, too obvious to need separate mention, are encountered in the other cases discussed above.

and looks deep into the field of vision thus presented, beyond the background as usually observed, one sees about the point of regard, after the field of vision has steadied, slowly moving swirls.[1] These swirls have the appearance of streams of granules moving in broad curves now this way, now that, seemingly without order unless a noticeable eye-movement occurs, or is made voluntarily, when the direction of streaming changes to that of the eye-movement.[2] The change of direction is always on a curve, the abruptness of which depends upon the vigor of the movements, much as would happen if motions in different directions and of different magnitudes were compounded at intervals upon a fluid of considerable inertia. The phenomenon is extremely varied. Sometimes the central portion of the field of vision resembles the surface of a liquid about to boil, channeled this way and that by convection cur-

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[1] The manner in which the lids are held is of extreme importance. They must not be closed so tightly that pressure is exerted on the eyeballs, and on the other hand, they must not admit light. It is difficult at first to find just the right background and the proper illumination. Quivering of the lids is fatal to the observation. The writer's only failure to secure a successful observation was on the part of an observer who could not keep the lids from trembling. Such painstaking precautions are necessary, however, only until the observer has once seen the phenomenon. Afterwards there is no difficulty. In fact, like the entoptic and circulation phenomena, the streaming may even become troublesome by its insistence. The plane of fixation is a matter of peculiar consequence. One must look through and beyond the shifting, changing, indefinite haze that occupies the visual foreground of the closed eyes. No better direction can be given than that the observer try to resolve this haze, to find out what lies in and behind it. He must gaze intently and penetratingly. Since the smoothest part of the closed lid lies above the centre of the eye, it is of advantage to look slightly upward, instead of directly forward. The steady field of vision should reveal the streaming, but voluntary eye-movement, by increasing its activity, frequently facilitates the observation. If one moves the eye sharply, and intently watches the field of vision in the trail of the movement, one sees (sometimes immediately following and sometimes lagging behind) a stream which takes the general direction of the movement. By way of final caution, we cannot emphasize too thoroughly the need of persistence and patience in observing. An unpractised observer should not expect to see the phenomenon in less than 2-5 minutes after closing his eyes. The field of vision must clear and settle, and the observer must grow accustomed to the unusual conditions of observation.

[2] The real movement of the streaming is in the opposite direction to that of the retina. The apparent motion of the eye is the movement of its anterior portion. This is opposite to the movement of the retina; hence the streaming material seems to pass across the field of vision in the direction in which the eye is moving. It might, of course, be expected that a mobile material on the retina would move under the influence of eye-movement as the streaming material is thus found to do.

rents moving at varying rates of speed. Now and again a heavy stream will sweep across this channeled surface from one direction or another, taking up the minor swirls as sharply curving tributaries, and so on, through manifold changes. Various patterns can be picked out, and a particular swirl may be traced in its deviations for a time; but, as a whole, the phenomenon cannot be adequately described.

After practice on the closed lids, the observers became able to trace the streaming on any dull or rough surface with the eyes open. It may also be observed under the conditions of observation of the entoptic and circulation phenomena; but just as one must look beyond the false scotomata to see the moving corpuscles and interspaces, so must one look beyond them to see the streaming. So competent did certain observers become, with the eyes open, that records were made in the experiments with minimal stimuli of the time that heavy streaming lasted during the phases of invisibility.[1]

2. In casting about for a physiological explanation of the streaming phenomenon, we have been led to believe that it is caused by a streaming over the retina of some material which is capable of directly affecting the processes that condition visual sensation. First, on the negative side, we find that it cannot be a circulation, entoptic, or tear-film phenomenon, or any of the shadow phenomena; for it is seen in the dark as well as in the light. In fact the best way to observe it, with the eyes open, is in the dark room or in a carefully muffled blackness cylinder. Secondly, on the positive side, we have three facts to consider. The streaming occurs, as we have just mentioned, in the dark as well as in the light. It is seen in the dark as a streaming and swirling of the intermingled blackness and luminous haze that compose the visual field. Here it must directly excite the black-white process. Again, the streams carry with them the quality of the background from which they proceed. This fact may be demonstrated as follows. Get upon the retina a large square blue after-image, having through its centre a vertical strip of yellow. Projected upon the field of vision afforded by the closed lids in daylight, this image will be seen as a strip of reddish yellow on a background of purple. A heavy stream, in passing across the strip, will sweep the purple with it across the yellow, and will deposit traces of the yellow in irregular patches on the further side. In other cases, where a heavy swirling takes place over the strip area, the yellow will break up irregularly, traces and

[1] The phenomenon comes out with remarkable clearness with open eyes, in the blackness of the dark room. The field of slightly luminous haze that there confronts one for some minutes, at a certain distance, streams and swirls with convincing distinctness.

patches of its color being borne out in different directions, and the background swept in. In yet other cases, the swirl will form outside the image, and sweep across it, much as a swirl of snow is carried before the wind. In all of these variations of stream-type, however, it is the unfailing rule that the quality of the background is carried by the stream from point to point in its course. Lastly, the streaming has a characteristic effect upon the after-image. Gentle streaming dims the after-image, apparently in proportion to its vigor, provided that it comes from an area of different visual quality, but heavy streaming blots it out absolutely. This occurrence, once seen, can never be doubted. The observation is positive.

3. The effect of streaming is the same, whether the eyes are open or closed. It is true that the stream is not so clearly seen to sweep over the after-image when this is projected on a background with the eyes open, as when it is projected on the field of the closed lids. Nevertheless, the instant that the image disappears, streaming can always be plainly seen over the area which it occupied. If the background is a rough, dull surface, the stream can even be seen to form and pass across the image. Most of our observers readily noticed this phenomenon, and some reported it before they had discovered streaming in the field of the closed lids, so that it formed their first observation of streaming.[1]

We have, further, indirect evidence of the identity of fluctuation under the two conditions of projection, in the striking

[1] A word of explanation here may prevent misunderstanding. What was observed, with the eyes open, was that the background seemed to sweep over this or that part of the after-image area, and that as the stream advanced the color disappeared. The stream seemed, decidedly, to sweep the color out, or at least to be causally connected with its disappearance. The phenomenon is, naturally, less striking than it is when the after-image is seen on the field of the closed lids. For one thing, the image is now projected on a field some distance away; the streams are thus magnified, and accordingly rendered vague and diffuse, less distinct in form and outline. For another thing, the brightness of daylight illumination tends to obscure the phenomenon. And other factors in the result could probably be mentioned.

Many of the brief disappearances of visual stimuli, especially those of positive stimuli of considerable intensity, are seen to be caused in the same way by streaming. The stream sweeps over them and blots them out; as it passes off, they reappear from behind, slightly farther back. The latter phenomenon is especially noticeable in the case of the shadows cast by a false scotoma on the retina.

Similar observations may be made in the course of adaptation experiments. Lay, *e. g.*, a fairly dark disc of Hering gray on a background of Hering gray several shades darker. While the disc is leveling down to the background, this or that portion of it will be repeatedly swept out by streams, moving across it in a curve. Many of the writer's laboratory students at different colleges have reported this phenomenon.

correspondence which obtains between the types of disappearance when the image is observed, as is ordinarily done, on a cardboard or other background, and when it is traced on the field of the closed lids in conjunction with streaming. Parallel series of experiments were carried out with after-images in the form of strips, squares, and crosses, large and small, projected both upon a background of cardboard and upon the field of the closed lids. The result was that, if a sufficient number of cases is considered, every type of disappearance found in the one series can be found also in the other. The following paired observations, selected at random, will illustrate this correspondence.

Stimulus: Milton Bradley standard yellow, 42 by 4 cm. After-image observed on background of engine-gray cardboard. Time of stimulation 30 sec. Distance of *O* 1 meter.

After-image dimmed over its whole area. Revived. Bottom went out. Reappeared. Whole image went, disappearing first along right edge in form of a curve, convex inwardly, then spreading. Reappeared, beginning along right edge. Top disappeared. Reappeared almost immediately. Top and bottom went out almost simultaneously, slowly followed by centre. (These disappearances rarely if ever occurred over the whole area at once. They began in a certain part, and then extended; one could see the area in process of being swept over. The parts disappearing were always bounded by curved lines.) Bottom came back first. Upper part disappeared, central and lower part dimmed, but did not disappear. Reappeared. Centre went out; then bottom. Quickly reappeared. Whole after-image disappeared, beginning in lower right-hand corner and spreading towards the top. Faintly reappeared. Vanished.

Stimulus: Milton-Bradley standard yellow, 42 by 4 cm. After-image observed on the field of the closed lids. Time of stimulation, 30 sec. Distance of *O* 1 meter.

Gentle streaming aud dimming over the whole area of the after-image. Upper half and centre swept out by a swirl moving counter clockwise, and carried across from the left. Reappeared. Whole after-image blotted out by two streams moving from left and right, joining, and moving upwards for the whole length of the after-image. Cleared over whole area, beginniug at bottom. Lower part swept out by a stream moving obliquely down and to left. Almost immediately another swept up and to right, carrying out centre and upper half. Reappeared. Swirl counter-clockwise cut off top. Reappeared. Stream moving on a very gradual curve to right from above cut out all but right edge of lower half. Finally this became involved. Cleared. Heavy stream moved across centre from right, blotting it out; divided and swept back on itself obliquely towards top and bottom, carrying out whole after-image. General commotion. Dimmed. Unable to follow changes farther.

Stimulus: Milton-Bradley yellow, 10 by 10 cm. After-image observed on engine-gray cardboard. Time of stimulation, 30 sec. Distance of *O* 1 meter.

Gradually dimmed. A curved segment was blotted out of left side. Reappeared. Disappeared, beginning with top. Reappeared, beginning with upper right-hand corner. Disappeared, beginning with top and upper left corner. Left side came back first, then whole image. Disappeared, beginning at top. Lower left corner went next.

Lower right corner was slow to go. Reappeared, beginning with lower left corner. Disappeared again almost immediately. Reappeared, beginning at bottom. Quickly disappeared. Reappeared faintly; then vanished.

Stimulus: Milton-Bradley yellow, 10 by 10 cm. After-image observed on field of closed lids. Time of stimulation, 30 sec. Distance of *O* 1 meter.

Gentle general streaming dimmed after-image. Lower right-hand corner cut off by streams, moving on short curves. Cleared. Stream from below moving from right to left swept out lower right corner and bottom. Cleared. Stream swept from right upper corner diagonally to lower left; immediately turning back and describing a sinuous path, swept out all but upper left and lower right-hand corners. They, too, were soon involved. Cleared, beginning at lower right and upper left corners. Two streams coming from below, left and right, joined and passed upwards across after-image, sweeping it out. They turned and apparently wound back and forth over after-image several times, causing a long disappearance. Lower left corner cleared first. Light stream swept across from left to right and almost blotted out image. It turned broadly upwards, then sharply downwards as a heavy stream, blotting out image completely. Cleared, beginning at upper left corner. General agitation over after-image (convection-current effect). Colors of background and after-image mingled in the general swirling. After-image disappeared. Cleared. Stream started at bottom, bent towards right, turned on itself towards left, again upwards and towards right, and then downwards in broad S-shaped curve. After-image was blotted out. Faintly reappeared. Almost immediately was involved in a general swirling and vanished.

The next two observations serve, further, to demonstrate that when a stream sweeps across an after-image area, causing the after-image to disappear, it carries with it the visual quality, color and brightness, of the area from which it proceeds.

Stimulus: a strip of Milton Bradley standard yellow paper, 42 by 4 cm. on a sheet of Milton-Bradley standard blue paper, 52 by 59 cm.; giving as after-image on the field of the closed lids, a reddish blue strip on a reddish yellow background. Time of stimulation 30 sec. Distance of *O* 1 meter.

First the yellow background could be seen streaming gently over the after-image, gradually dimming it. Swirl of complicated curves blotted out whole after-image. Cleared slowly, and after-image could be seen here and there at the less dense places. Gradually cleared all over. Circular swirl cut out centre and about two-thirds of upper half. Cleared. Another swirl cut out small area at centre and moved diagonally upwards and towards right. Centre cleared. Streams, sweeping from right to left, joined at bottom of after-image, and traversed its entire length, returning upon themselves on curves having the shape of an ellipse. After-image cleared beginning at bottom. Lower part of after-image cut off by stream sweeping across to right, which turned and went obliquely towards left and upwards, cutting out centre. Cleared at bottom first. General swirling. At some places after-image was carried out into background, and background carried in; so that after-image presented irregular outline. Soon swirling became more violent and after-image and background intermingled. After-image became indistinguishable. Did not reappear. (In every disappearance the stream could be seen to carry the yellow of the background over the strip. This was especially

noticeable when the disappearance was progressive, from part to part. When, *e. g.*, a swirl, expanding, involved successively more and more of the image, the front of swirling yellow could be seen to encroach upon the blue at each revolution.)

Stimulus: a strip of Milton-Bradley standard yellow paper, 42 by 4 cm. on a sheet of Milton-Bradley standard blue paper, 52 by 59 cm.; giving as after-image on a sheet of engine-gray cardboard a strip of blue on a background of yellow. Time of stimulation 30 sec. Distance of *O* 1 meter.

Dimmed all over. Went out at centre, then at top and bottom. Reappeared. Bottom swept out; then whole image. Reappeared. Section in middle of upper half went out, followed almost immediately by section in middle of lower half. Both reappeared. Whole after-image blotted out. Reappeared, first at bottom. Top and bottom went out, followed by centre. Centre reappeared first. Long, slender, crescent-shaped section disappeared from right hand upper, then from left hand lower portion. Reappeared faintly, beginning with right hand upper corner; then whole image disappeared. Reappeared. Lower part disappeared. Reappeared momentarily; then whole image vanished. Reappeared faintly, and vanished. (At each disappearance, the after-image area occupied by the blue strip took on the yellow of the surrounding background, instead of preserving the gray of the cardboard. In many cases it could be seen to do this progressively; *i. e.*, the yellow would begin at a corner, an edge, etc., and spread in the direction in which the disappearance of the strip was taking place. The phenomenon was very clear, *e. g.*, when a corner, bottom, or what not went out, and the disappearance spread, finally involving the whole image. This type of disappearance, when observed on the closed lids, generally showed a swirling stream which spread centrifugally until the whole after-image area was swept over.)

4. The effect of streaming on the flight of colors is threefold. First, a gentle streaming may merely dim the color. Secondly, more intensive streaming advances the color changes one or more stages. When the change is advanced only one stage, the image sometimes does, and sometimes does not return to the preceding stage, when the stream has passed over. When the change is advanced two or more stages, the image apparently always returns to the initial stage, or to the initial stage but one, when the stream has cleared away. Thirdly, when the streams are strong and heavy, the image is blotted out completely, returning to the initial stage when the stream has passed on either abruptly, or quickly through the series of color changes,—most frequently in their inverse order, but sometimes in irregular order. These effects cannot be due to a shadow cast by the stream upon the retina; for shadows, like any reduction of the illumination of the retina, push the color change in the inverse direction. Hence we have here additional evidence that the locus of the physiological condition of streaming is not anterior to the retina.

The following is a description of the effect of streaming on the flight of colors.[1] An account of the phenomenon as it

[1] All the phenomena described in connection with streaming are

appears under still better conditions has been given above (after-image from the sun's disc).

The stimulus was afforded by a triangular opening, 1 by 2 ft., in a curtain near the ceiling of a closely curtained room. *O* was seated about 10 ft. from this opening, and looked up through it directly at the brightly illuminated sky. The sash of the window was lowered. In line with the centre of the opening was a patch of bright, fleecy clouds. Thus the stimulus consisted of a brilliant blue triangle with a bright white patch at its centre. When stared at for a long time, *e. g.*, for 5 min., the triangle underwent the following qualitative changes. The white patch at its centre became gradually dimmer. Finally, it became completely overcast with the surrounding blue. This was concomitant with intensive streaming of the swirling type. It cleared somewhat; then the whole triangle changed to a deep, saturated pink. After remaining in this stage for a short time, it changed again to blue, and finally once more to pink. Here the observation ceased. When the after-image was to be traced, *O* fixated the centre of the opening for about 20 sec., and then covered the closed eyes with a black cloth. The observation was thus made in a well-darkened field of vision. The report is as follows.

After-image developed as light faintly reddish blue. Fluctuated several times between this and yellowish green. (Thus far, as it happened, the streaming was not intensive.) Finally settled into yellowish green. Heavy streams frequently swept across, carrying it completely out. On reappearing, it came directly back to yellowish green once. The remaining times it reappeared first as deep red, then quickly changed into yellowish green. The yellowish green area grew smaller as fluctuation went on, leaving a growing border of deep red. Next fluctuation began from yellowish green to deep red, as a result of less intensive streaming. The yellowish green area was swept off, as if it were an upper layer, leaving the purplish red underneath and further back. One could see the yellowish green streaming raggedly beyond the after-image. This was very plain at times. Once a narrow stream was observed to cut a channel through the yellowish green near the toe of triangle, exposing the deep red apparently beneath. Disappearances also took place at this stage as result of heavy streaming. After-image finally settled down into deep red. This layer seemed to be noticeably farther back in the field of vision than the other color layers. Disappearances were quite frequent, and were plainly the result of streaming. This stage lasted relatively a long time. Next faint stages respectively of dark blue and dull dark yellow were noticeable. Fluctuations were frequent and disappearances long. There was the usual connection between fluctuation and streaming.

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noticeably plainer when the observation is made in the higher altitudes. Whether this result is due solely to the condition of illumination there found, which facilitates observation (especially when made in daylight on the field of the closed lids), and conceivably induces a more or less special retinal state, or whether indirect physiological influences are at work, the writer has no means of deciding.

The following is a variation of the streaming phenomenon, not observed in connection with after-images. It is given as possibly throwing some light on the physiological condition of streaming from a slightly different angle. The observation was made out of doors in Colorado, early in October, near the middle of a cloudless day. The light stimulation was very intensive.

Sometimes when the eyes, having been exposed to the bright diffuse light, were closed, and the field of vision had settled, one saw scattered over it here and there, and fairly close together, islands or patches slightly darker than the background, presenting a porous appearance, due to a peculiar mottling of lighter and darker gray. These patches were not after-images. They were, however, readily seen to be carried along and broken up by the streams, the parts taking the velocity of the stream current. Streams could be seen to cut channels through groups of the patches. The patches themselves seem to be conditioned by some mobile material, which shares in the general streaming.

Another phenomenon, which seems to indicate that there is even a mass mobility of the material which conditions visual sensation, is described as follows. The observation was made under the same conditions as the last.

When one has two or more after-images of the sun's disc close together, they will often be seen soon to merge into one. Sometimes a channel cuts across from the one to the other, and the two gradually draw together into a more or less circular form. Again, the one will be seen to move bodily towards the other, finally uniting with it.

5. That there is a dependence of streaming upon eye-movement cannot be doubted. This dependence is shown in two ways. First, when an eye-movement is noticeable, or is made voluntarily, a heavier stream is started in the apparent tangle of swirling in the direction of the eye-movement. Secondly, whenever involuntary eye-movement is increased, as by previous long fixation, by a faulty centering of the after-image upon the retina, etc., greater commotion is noticeable over the streaming area. That there is a movement of this material, independent of the effect of eye-movement, is also probable; but the heavy streams that intermittently blot out the after-image are doubtless determined by eye-movement.[1]

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[1]What the nature of the streaming material is we shall not attempt to decide, further than to point out that the results show it to be visually active. Metabolism requires that there be a diffusion of lymph over the retina. We might, then, identify the streaming material with this metabolic substance, making it the vehicle of both catabolic and anabolic processes. The anabolic material is conceivably in part disintegrated visual substance which retains for a time its power to condition visual sensation, as is shown by the streams bearing with them the visual quality of the region from which they come. Thus by weakening the after-image through hastening metabolic change, and by setting up strongly the sensation of the region from which the

c. When once we have established the connection between fluctuation and eye-movement on the one hand, and eye-movement and streaming on the other, explanation goes comparatively smoothly. The results obtained by varying the steadiness of fixation and by increasing the time of stimulation present no especial problem. There is in these cases merely an increase or decrease of eye-movement, and a corresponding increase or decrease in the streaming activity, with a resultant increase or decrease in fluctuation. With the remaining methods, however, the situation is different. They will be considered in turn.

(1) We have found that fluctuation occurs only within a limited region of after-image areas. Probably the most difficult problem that fluctuation sets to theory is this effect of variation of area. Before it, the oscillatory theory seems to break down absolutely.

Considered with regard to area, after-images naturally fall into four classes: small images which fluctuate little or not at all (for our observers, images with an area of 1.5 by 1.5 cm. and less); larger images which fluctuate over their whole area; still larger images which fluctuate in parts; and quite large images which do not fluctuate at all. Now this grouping is just what we might expect from the nature of the streaming phenomenon. It derives directly from the following facts: that streaming does not cause disappearance unless the stream comes from a part of the retina undergoing different stimulation; that the streams have a more or less definite form, *i. e.*, that they sweep across this or that part of the after-image, preserving pretty clearly defined borders; that vigorous streaming apparently occurs only over a somewhat limited region about the point of regard; and that the centre of the field of vision is always in a state of more or less violent swirling. Here the streams are narrow and swift-moving;[1] as we pass towards

streams come, heavy streaming may temporarily obscure the after-image. It is, however, useless to work out in detail what, with our present knowledge of visual processes, can at best be but a mere conjecture. We desire to lay stress upon nothing except the observed facts.

[1] The direction of streaming is, as we have seen, determined or modified by eye-movement. The lines of direction described by all the points on the moving retina, for the various directions in which the eye moves, crowd together in its central portion; hence all the streams, since their directions are determined by the moving retina, converge towards its centre. This accounts for the narrowing of the streams, and the consequent more rapid motion of the streaming material. More or less continuous commotion at the centre must also result from these conditions; and the twisted, swirling, tangled patterns are produced by the many-times compounded motions. The centre of the retina is thus least liable to adaptation and after-image

the periphery, the swirling becomes more diffuse, and the streams are broader, and more widely separated both in space and time.

For convenience of discussion, therefore, the retinal field may be divided into four zones, each one of which usually, but not always, contains different stages of the same stream. The streams form near the periphery of the retina, and tend to move towards its centre. In the first or central zone, streaming is practically continuous. Here the streams are narrow, swift, and often crowded together. The second zone is made up in part of cross-sections of the streams found in the first, and in part of streams whose directions have been changed before they reached the first zone. Here the streams are broader and less swift and vigorous. The streaming at any given point is discontinuous; a given segment of the zone is streamed over at irregular intervals. In the third zone are found, in general, the source and the upper courses of the streams passing across the first and second zones. The streaming here is still more discontinuous, and the streams are still broader and more sluggish. The fourth zone lies beyond the area of observable streaming.

The infrequency or entire absence of fluctuation of the small images of the first group is explained by the fact that they lie wholly within the first zone, the region of most active commotion. Hence, when they have once reached the dimness at which disappearance begins, there is not sufficient lull in the streaming for reappearance to occur. The records show less eye-movement for the images of the first than for those of the second group. Here we come upon an exception to the general law of the effect of eye-movement upon fluctuation; for this reduction of eye-movement favors rather than decreases the fluctuation of these small images. It is, however, readily intelligible that a more continuous eye-movement, by causing more continuous streaming, would give even less opportunity for reappearance.

The images of the second group extend beyond the limits of the central zone; and the continuous streaming of this region does not cause them to disappear, since to do so the streams must come from a region of different stimulation. They lie, however, within the second zone, where the streaming is still vigorous though not continuous. A broad heavy stream sweeps in from the outside, and may either blot out the whole image at once (this depends upon the relative sizes of stream

effects. This conclusion tallies with well-known facts of vision. The centre of the retina is the region of clearest vision; adaptation there takes place less quickly, and the correlated after-images develop more slowly.

and image), or may at first efface only a part. Then, as the stream compounds with other streams, the whole image will become involved. When the commotion subsides, the image reappears, remaining until blotted out by another stream or stream-system. Since we are now in a region of only occasional streaming, increase of eye-movement must increase the frequency of fluctuation, by increasing the number of streams that sweep across the image. There is, however, sufficient intermission in the streaming for reappearance to take place. Continuous voluntary eye-movement would probably produce continuous streaming over this region; but, under the conditions of ordinary fixation, we have a fluctuation which is proportional to the frequency and range of eye-movement.

The images of the third group are still included in the region of streaming. They lie within the borders of the third zone, but are too large to have their whole area involved at one time by a single stream or stream-system. So we have the phenomenon of fluctuation in parts. As in the previous case, increase of eye-movement increases streaming, and accordingly increases frequency of fluctuation.

The images of the fourth group cover the whole area of effective streaming. Their borders lie in the fourth zone; and consequently fluctuation does not occur. The streams that twist about over the surfaces of these images do not come from a region of different stimulation, consequently do not blot them out.[1]

(2) We found that the form of the stimulus affects the frequency of fluctuation and the duration of the after-image. The stimuli used were strips and squares of equivalent areas. We now have to explain, first, the shape of the curve of frequency for the strip-images. An increased length of strip, while it produces practically the same effect on eye-movement as increased area of square, does not give the same shape to the curve. With the squares the curve, after reaching its maximal height, bends down rather sharply to the abscissæ;

[1] The explanation in this and the following sections may seem somewhat complicated; but the facts are themselves complicated. The results of observation are recorded as they were obtained, in no wise modified to suit the needs of theory. The phenomena of streaming and the phenomena of fluctuation were investigated independently and at different times. The details of streaming, its patterns, zones, etc., were worked out, and in part verified, a full year before the results on fluctuation given in the foregoing tables were obtained. After those results had been obtained, however, for variations in size, form, arrangement, etc., of stimulus, the present investigations were begun, with projection of the images on the field of the closed lids, in order to determine the relation of the various types of fluctuation to the various types of streaming. Thus our theory is, in reality, a description of what actually takes place in observation.

with the strips, it dips down very little. The explanation is that the squares, as they grow larger, come to include the whole of the noticeable streaming area, while the strips do not. The strips can, accordingly, always be swept across by streams coming from a region of different stimulation. They can be blotted out, while the squares cannot. The shape of the curve for the strips is very like that for the squares until the maximal height is reached; up to this point streaming affects both strips and squares alike.

We have to explain, secondly, the fluctuation of strips and the partial absence of fluctuation of squares of equivalent area. The explanation lies in the difference of the retinal zones. The squares fall within the first and the innermost part of the second zone. The strips, in proportion as they are included within the first, second and third zones, show the phenomena of fluctuation characteristic of these regions.

(3) We found that the arrangement of the stimulus, with reference to the direction of greatest eye-movement, affects the frequency of fluctuation and the duration of the after-image. Narrow strips of varying length undergo more frequent fluctuations and have a shorter duration when the direction of greatest range and frequency of eye-movement is across the strip, than when the inverse arrangement obtains. The reason is clear: the streams must be more effective to produce disappearance when they sweep across the narrow after-image, than when they traverse its length. Suppose, *e. g.*, that a narrow strip is placed with its length first in the vertical and then in the horizontal plane. Let the eye-movement, in both cases, take place in the horizontal plane. The vertical strip can be more effectively swept by streams coming from a region of different stimulation than can the horizontal strip. Accordingly we find greater frequency of fluctuation and a shorter duration of the image in the former case than in the latter. Conversely, when the movement is in the vertical plane, and the strip is arranged first vertically and then horizontally, the opposite effect should be produced. The tables show that this is the case.

In the experiments on natural fluctuations, the greater range and frequency of eye-movement, for all observers, were in the horizontal plane. Hence greater frequency of fluctuation and shorter duration should have been observed when the strip was arranged with its length in the vertical plane. The tables show that this expectation was realized.[1]

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[1] The character of the disappearance is somewhat different, according as it is due to voluntary eye-movement or to natural fluctuation. In the former event it is more abrupt, and more nearly covers the whole length of the image. In natural fluctuation, the image usually

(4) Having thus ascertained the part played by streaming in the determination of the duration and fluctuation of the after-image, we can understand how it is that results obtained when the fluctuations were produced by involuntary eye-movement, varying in amount from method to method, could be duplicated by results obtained when the fluctuations were produced by voluntary eye-movement, constant from method to method. There are two possible reasons. First, there was present in both cases a variable amount of involuntary eye-movement; and secondly there was in both cases the same distribution of the zones of streaming.

There can be no doubt that voluntary eye-movement, while it lessened, did not entirely prevent involuntary movement. In the variation of this latter, from method to method, we might find a basis for the variation in results obtained, and therefore an explanation of the duplication. On the other hand, there is strong evidence that the concomitant involuntary eye-movement was not the direct cause of fluctuation. The disappearances always followed directly upon the voluntary movements, which must, therefore, be regarded as the immediate cause of fluctuation. The involuntary movements could have functioned only indirectly, by way of weakening the after-image, under certain experimental conditions, and thus rendering it more liable to obliteration by the voluntary movements. Obviously, then, the distribution of the zones of streaming is the more important factor.

It is not difficult to see how these two factors co-operated to produce our results. If there were no variation of involuntary eye-movement, from method to method, strict duplication should result when the fluctuations are produced by voluntary eye-movement. If it were not for the identical distribution of the zones of streaming, duplication could not result at all. A consideration of the two factors together enables us to explain the results obtained.

(5) The peculiarities of fluctuation in indirect vision are readily explained as a result of the distribution of the zones of

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goes out in successive parts, quickly, until it has disappeared. The difference reflects the nature of the eye-movement. The voluntary movement is a single sweep, out and back, of considerable strength and range. A broad current of the streaming material is thus carried across the image in the direction of the eye-movement, and blots it out at once. The involuntary movements occurring in the case of natural fluctuation are irregular in direction, range and frequency, and usually come in groups. They therefore start a number of streams in different directions, and usually in quick succession. One stream is often seen to sweep across this part, another across that; until finally the whole image becomes involved before any part has had time to clear.

streaming. When a small after-image is observed, first in the central part of the field, and then successively farther and farther out from the centre, there is first an increase in fluctuation, then a decrease, and finally an entire cessation. Now we have seen that an image in the central zone of streaming, once it has disappeared, is kept from reappearing by the continuous commotion there present. As it passes from the central zone outward, into the region of occasional streaming, fluctuation must increase up to a certain point (probably the limits of the second zone), and thereafter decrease, ceasing entirely when the image passes beyond the range of noticeable streaming.

(6) Fechner,[1] Helmholtz,[2] and others maintain that blinking and movement of the head, as well as movement of the eyes, cause the after-image to disappear. Both of these movements, however, result in eye-movement, and hence may be supposed to be only indirectly causes of disappearance.

With regard to blinking, O. Weiss says:[3] "Beim Lidschluss zeigt sich eine Bewegung des Bulbus erst nach oben innen, dann nach oben aussen." This is called Bell's phenomenon.[4] The movement can easily be felt when one presses the finger with moderate firmness on the lids, above and to the temporal side of each bulb, and blinks vigorously. Von Michel[5] thinks that this movement-complex is controlled by the cortex, while Nagel[6] believes it to be a reflex, due to the pressure of the edges of the lids upon the cornea. However this may be, there is distinct eye-movement, in at least two directions, with every closing and opening of the lids; that is to say, there is ample ground for considering eye-movement to be the more immediate cause of the disappearance of the after-image.

Again, even if the eye were stationary in its socket, movement of the head would affect the streaming phenomenon very much as movement of the eyes does. The streaming material would traverse the retina in the opposite direction to that of the movement.[7] But the eye is not thus stationary; movement of the head results either in a movement of the eye in the opposite direction, or in this together with a readjustment of the eye in accordance with the changed position of the head. And there is, further, a rotation of the eye about its horizontal axis, which, according to Donders, opposes the rotation of the head

[1] Ann. d. Phys. u. Chem., L, 1840, 221.
[2] Phys. Optik, 510.
[3] Nagel's Handbuch d. Physiol. des Menschen, III, 1905, 471.
[4] Philos. Transact. of the Royal Soc., 1823, 166, 289.
[5] Beitr. z. Physiol., Festschr. f. Fick, 1899, 159.
[6] Archiv f. Augenheilk., XLIII, 199.
[7] This would be in the same direction as the movement of the field of vision, the converse of what happens with eye-movement. The effect on the after-image, however, would be essentially the same.

and is of equal amount in both eyes. In the case of a sudden inclination of the head, Mulder[1] found a momentary torsion of 20°. His conclusions as regards permanent torsion bear out those of Skrebitzky.[2] Nagel[3] found that movements of torsion occur if the head or the head and body together are passively moved.

In fine, then, the effect upon the after-image of blinking and of movements of the head presents no especial problem to theory; in both cases definite and measureable eye-movements take place. Eye-movement, as determining or modifying the streaming phenomenon, explains fluctuation under these conditions as readily as it explains the fluctuations which occur under the conditions of normal fixation.

III. Conclusions and Restatement of Thesis.

The conclusions to be drawn from the foregoing experiments, with regard to the fluctuation and duration of the negative after-image, are as follows. (1) The fluctuation of the negative after-image represents a real intermission of sensation. It is not an artifact, due to observation under the conditions of light adaptation, for it occurs as readily in a darkened as in a light field of vision. (2) Fluctuation is not grounded in the nature of the after-image process. It is caused chiefly by involuntary eye-movement. (3) Eye-movement causes the fluctuation and decreases the duration of the negative after-image by conditioning or modifying the streaming over the retina of some material capable of affecting the visual processes.

Enlarged and restated in the light of these conclusions our thesis is this. (1) The intermittence of minimal visual sensation is a phenomenon of adaptation. (2) Adaptation is rendered intermittent chiefly through the influence of eye-movement. (3) Eye-movement interferes with adaptation in three ways. (*a*) It decreases the total time of stimulation. The more eye-movement there is, the less intensive will be the impression made upon the retina. (*b*) It affords time for the after-image to die away, or (in terms of adaptation) it gives opportunity for restoration, proportional to the length of time during which the stimulated area is relieved. And (*c*) more immediately, it determines or influences the washing or streaming over the retina of some material capable of directly affecting the visual processes. Further evidence for this thesis will be adduced in later papers.

[1] Archiv f. Opthalmol., XXI, 1, 1875, 68.
[2] Archiv f. Opthalmol., XVII, 1, 1871, 107.
[3] Archiv f. Opthalmol., XVII, 1, 1871, 237.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

COMMUNICATED BY EDMUND C. SANFORD

XXI. A PRELIMINARY REPORT OF EXPERIMENTS ON TIME RELATIONS IN BINOCULAR VISION[1]

By TIMOTHY J. STEVENSON and E. C. SANFORD

The stereoscope, as ordinarily arranged, presents at the same time to each eye an appropriate picture. In a special form, devised by Prof. Münsterberg, however, the pictures are not presented simultaneously, but each eye sees its own picture for an instant while the other eye's picture is covered.[2] Still earlier, Dvorák showed that stereoscopic vision was possible under circumstances similar to these, and described certain illusions depending on that fact,[3] Dvorák, and later Exner, studied also the least time interval that could be observed between visual stimuli offered separately to the two eyes.[4] The study now to be described belongs to this general group, but the particular questions with which it deals are slightly different from any of these. The first problem undertaken was that of determining what time interval, if any, may be inserted between the presentation of each picture to its proper eye without marked interference with the perception of stereoscopic relief; and the preliminary results to be stated have to do chiefly with what is to be observed when the interval is made so long that stereoscopic vision becomes difficult or impossible.

The Apparatus. The apparatus used was in general terms, a Wheatstone stereoscope, fitted with notched disks in such a way that one eye's picture was first exhibited automatically

[1] In view of the fact that Mr. Stevenson is no longer working in the laboratory and that the problem is about to be taken up by other hands, it has seemed best to make this report of results which seem to be demonstrated.

[2] Münsterberg: *Psy. Rev.*, I, 1894, 56-60, Studies from the Harvard Psychological Laboratory—a Stereoscope without Mirrors or Prisms.

[3] Mach: Analyse der Empfindungen, 1903, p. 196. Dvorák: Ueber Analoga der persönlichen Differenz, Sitz.-ber. k. böhm. Ges. d. Wiss. (math.-nat. Classe, März 8, 1872).

[4] Exner: Experimentelle Untersuchung der einfachsten psychischen Processe. Pflüger's Archiv, XI, 1875, 402 f.

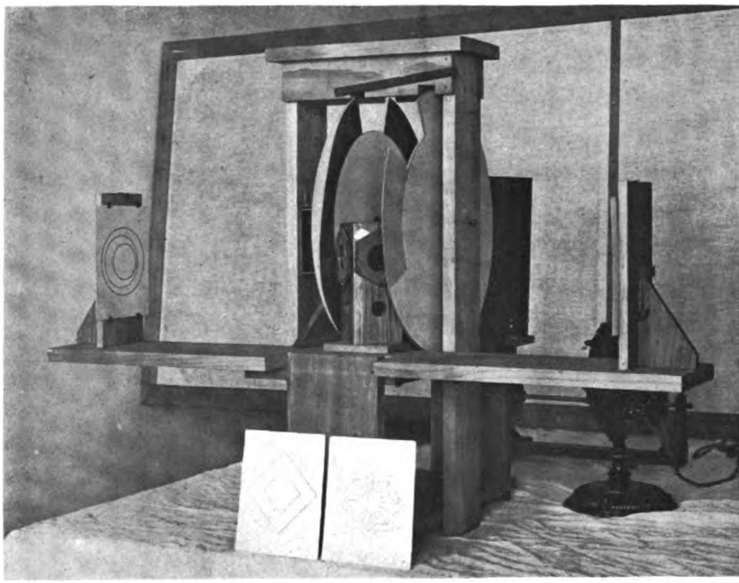

PLATE I

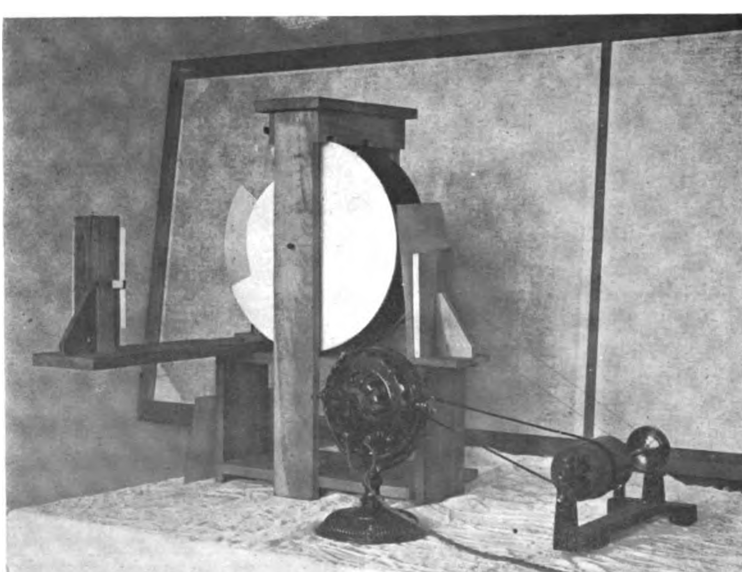

PLATE II

for a brief time, and then, after a known time interval, the other eye's picture was similarly exhibited for a time equal to the exhibition of the first eye's picture. The details of the arrangement will be made sufficiently clear by the accompanying plates and plans.

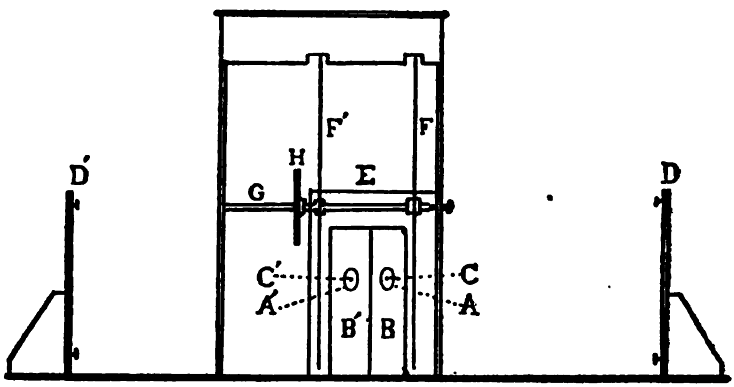

Fig. I

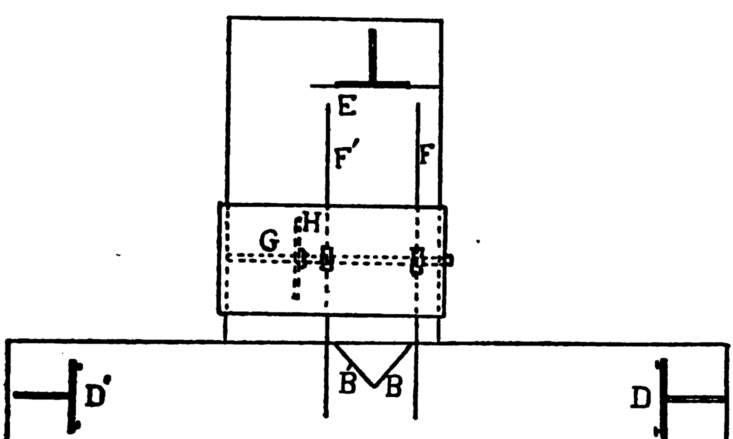

Fig. II

Plate I shows the apparatus as viewed from in front; Plate II, the same viewed from the side. Fig. I is a front elevation of the apparatus, and Fig. II its ground plan. *A'* and *A* in Fig. I are two plane mirrors, each about an inch and one-half in diameter, fastened over corresponding holes in two upright

panels, B' and B, standing at right angles to each other (Figs. I and II). In order to secure easy and perfect fixation, the silvering of each mirror was removed from a little spot at the centre, through which the observer looked at a white fixation point on the black background, E, which was itself carefully set so as to lie in a definite plane of the stereoscopic solid. Into coincidence with the fixation point just mentioned were also brought certain symmetrical points of the reflected images of the diagrams, carried by the panels, D' and D—usually the centre points of the larger circle or polygon.

The rotating disks used to secure the separate presentations of the diagrams are indicated by F' and F (Figs. I and II). These disks were 16 inches in diameter, and stood 4.5 inches apart. In the edge of each was cut a square notch, 30° wide and 2.5 inches deep. The disks were carried by a horizontal axis, G, lying parallel to a line joining the centres of rotation of the eyes of the observer, and 6.5 inches behind the centre of the mirrors. On this axis they were held by nuts which allowed them to be moved stiffly with reference to each other, and made it possible to set the notches in them quickly at any angular distance from each other up to 150° (180° minus 30°). The precision of the setting of the disks was tested by means of a straight edge attached to the frame of the apparatus, and a scale of degrees marked off on one of the disks.

The disks were rotated by an ordinary fan-motor, actuated by the commercial alternating current of the city, working through a series of pulleys to reduce the speed. The final pulley upon the axis of the disks is shown at H in the figures. This pulley was connected with the rest of the driving apparatus by a loose belt, running over an idle pulley serving also as a belt tightener. By this means the disks could be stopped or started as often as necessary without interrupting the movement of the rest of the driving apparatus. The rate of such a motor is usually quite constant, and in this case was tested before and after each experimental sitting (except one) by three timings of one hundred revolutions of the disks, each taken with a stop watch reading to one-fifth of a second.[1] The average rate found was slightly over 80 seconds per hundred turns. The greatest variation found in any single group of three countings was 0.4 seconds, and the extreme variation between timings taken in different days was 3.2 seconds. More exactly, the average time for one rotation of the disk was 0.804 seconds,

[1] On one or two occasions during the experiments, the motor clearly slowed up for a few seconds at a time, because of a temporary interruption of the current, or for some similar cause. At such times observations were, of course, suspended until the normal speed was resumed.

and thus for one degree 2.23σ; for 10°, 22.3σ; for 20°, 45σ; for 30°, 67σ; for 60°, 134σ; for 90°, 201σ, and for 150°, 335σ.

In most of the experiments here to be considered, the right hand picture of the pair was shown first, except of course for the setting at 150°, when the pictures simply alternated at intervals of 335σ measured from the end of the period of visibility of one to the beginning of that of the next. The exposure time for each picture was constant at 67σ seconds (30°) or in common fractions, about 1-15 of a second.

One each of the three pairs of stereoscopic diagrams used is to be seen in Plate I. The one in the holder at the left and the right hand one of those in front of the instrument (designated in the notes as Diagrams 13 and 15) are familiar forms. The inner and middle circle of Diagram 13, however, were colored; the inner one red, the next violet, while the outermost was black. This difference in color was unnecessary and a disadvantage, as it possibly tended to suggest monocular relief in ways which would not have been present with figures all in black. Diagram 15 was drawn entirely in black, but was unsatisfactory for another reason; its oblique lines introduced definite tendencies to monocular relief, according as one or another of them was fixated, and these monocular tendencies sometimes co-operated with and sometimes opposed the binocular relief. The other figure (known in the notes as Diagram 14 and shown in Figure III) is a less common one, copied from

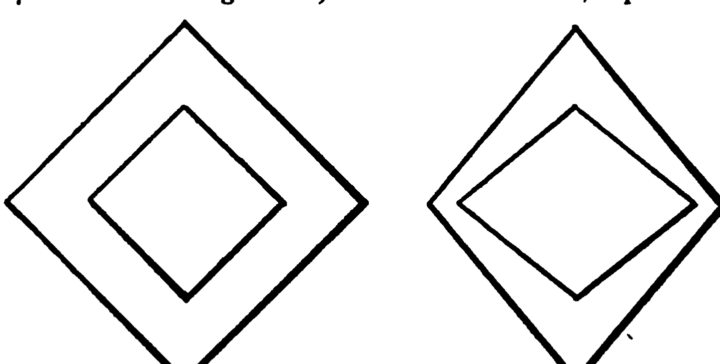

FIG. III

a collection of figures belonging to the Clark Laboratory. To those unfamiliar with it, this figure does not usually suggest, when uncombined, the form brought out by combination. When examined stereoscopically the diagrams show two diamond shaped planes intersecting at an angle of nearly 90°.

Reversal of any of these diagrams in the holders, of course,

brings about reversal in the apparent relief. It was easy, therefore, with a little care to begin a test without the observer's knowing what kind of relief he was to expect, and this was the usual procedure.

Of the large disks three sorts were used; one of black cardboard, one of white (the back of the black disks in this case, which gave a fair, but not exceptionally intense white), and one of a medium gray. These differences in brightness are by no means unimportant, for the disks not only shut off the vision of the pictures after the required interval of exposure, but the light which they reflect furnishes a general stimulation of the retina which has a profound effect upon the duration of the after-images of the diagrams. The illumination was ordinary day-light and, on several of the days, when it was necessary to experiment, varied considerably.

Method. The experimental procedure in the case of the more systematic experiments was as follows: A pair of the diagrams was adjusted in the holders and covered up to prevent the observer from knowing what form of relief to expect. The disks were then set with their notches at a considerable angular distance (usually 60°, though sometimes 90° or 150° was chosen), and put in motion. The observer took his place at the instrument, looked at the background and reported what he was able to observe. The experimenter, meanwhile, recorded the observations, asking questions when necessary to make the record definite. The observer was allowed to adjust his head and to use his eyes in the way most convenient to him. Occasional differences, due to fixation and movement, were reported. After observations with a large interval, the smaller were used in regular sequence: 30°, 20°, 10°, 0°, and —30°. The numbering of the scale of degrees was arranged to show the interval between the ending of the exhibition of the first picture and the beginning of that of the second. Zero degrees, therefore, means that one exhibition began the instant the other ended, and —30° means that the exhibition of both began at the same instant—that they were strictly simultaneous.

When observations had been made upon one pair of diagrams at the standard intervals, another pair was substituted and the process repeated. Similarly, when all the diagrams had been worked through with one pair of disks, another pair of disks was placed upon the axis, and the observation of all the diagrams at all intervals was again undertaken. The matters to which observation was especially directed were the nature of the stereoscopic relief, the apparent movement of the figures, and the character of the after-images.

Of the two chief observers, one (S) has had quite a little experience with work of this kind. The other (T) has had

only a small amount of general training in the psychological laboratory. A third observer (P) was, like the first, experienced in psychological experimentation. The note books contain one full set of observations for S and T with all the disks and all the diagrams, besides the records of some incidental testing taken while the apparatus was being perfected and at odd times during the taking of the regular set. For P, the records were taken with all the diagrams, but part with the gray and part with the white disks.

Results. The small number of systematic experiments and the complexity of the phenomena involved preclude our making any quantitative answer to our first problem, except a negative one. It is qualitative statements chiefly that we can make, and these for the most part with reference to what happens when the interval between the exhibitions of the two pictures is so long as to make stereoscopic vision difficult or impossible.

Our experiments have led us, however, to the conviction that *practically no interval whatever can be inserted between the periods of excitation of the two retinas* (including the duration of the after-image as a part of the first excitation) without destruction of the stereoscopic relief. It seems likely, on the contrary, that stereoscopic vision depends strictly upon the simultaneous excitation of the two halves of the visual apparatus.[1] Further systematic experimentation on this point may show that a very minute interval can be inserted, but it will certainly prove small even in comparison with the duration of the positive after-image.

Upon the following points, however, our observations have been so uniform and consistent that we feel justified in making a definite report with regard to them.

1. When the interval between the pictures is great enough to interfere with the usual stereoscopic apprehension of the diagrams, *their parts are apperceived as in motion.* They seem to change quickly from the position they occupy in one eye's diagram to that which they occupy in the other. This is especially true of the parts that fall upon non-corresponding retinal points, but it is also true to a certain extent of the parts that fall upon corresponding points, perhaps as a result of the relative way in which movement is usually perceived. Double images in the ordinary sense are not seen at such times and indeed cannot be seen until the periods of retinal excitation overlap and are thus, in part at least, simultaneous.

For Diagrams 13 and 15 this movement was chiefly a change

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[1] In Prof. Münsterberg's instrument, the positive after-images probably outlasted the entire interval.

of place, a sudden sliding or leaping from side to side of the inner parts of the figure, the diagrams having been so adjusted as to bring the outer parts approximately upon corresponding points. This movement was not always strictly horizontal but at times in an oblique direction, though there was nothing in the position of the diagrams or the observer that would easily suggest a reason. In Diagram 14 the movement was an apparent change in the shape of the figure itself, diamond to square or *vice versa*. This movement of the parts of the diagram was most striking with the longer intervals, and gradually decreased as the intervals were taken shorter and shorter, until it finally disappeared when the setting reached 0° or —30°. In some cases, the movement was apperceived as an excentric rotation in the plane of the background instead of a leaping from side to side; and in a few cases, when the interval was short, the movement seemed to be to and from the observer in the third dimension, instead of simply from side to side. In most cases, however, the movement and the binocular relief seemed reciprocal phenomena, the one decreasing as the other increases. When the conditions are not present for the adequate perception of relief, the apperceptive process takes on automatically another form, the perception of movement. In rare cases they are partially combined.

2. *As the interval is shortened and the conditions become more favorable to the apprehension of stereoscopic relief, the relief seems not to come in suddenly in full amount, but gradually.* Let us say, for example, that, in Diagram 14 the relations are such that under the conditions of ordinary stereoscopy the right hand point of the inner diamond would appear two inches in front of the right hand point of the outer diamond. Then with the gradually decreasing intervals of our experiment it would appear at first, perhaps, only half an inch in front and would gradually draw forward as the time between the pictures lessened until with the 0° or —30° settings it would be seen at full two inches in front. A corresponding but opposite change in the amount of the relief was to be observed when the interval between presentations was gradually lengthened from simultaneity.[1]

The gradual alteration of the amount of stereoscopic relief

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[1] It may be noted also in passing that it was usually the *advance* of some part of the figure from the plane of the fixation point *toward* the observer that was first and most easily to be observed. The location of the portions back of that plane seemed more difficult and was slower in coming to observation. A similar relation was demonstrated to one of us by Dr. J. Carleton Bell, in the Wellesley Laboratory, some weeks before our experiments began. We note in passing this confirmation of a result already obtained by him.

just mentioned, seems to us of some theoretic interest and so far as it goes to furnish an argument in favor of the theory that the perception of stereoscopic relief is due to a reflex tendency of the eyes to move so as to fixate the different portions of an object seen in relief (a tendency not by any means perceived as such, but simply in its result, the binocular relief), and especially to support Wundt's account of the nature of the complex local signs in binocular vision. If the matter were a reflex one, we might very well suppose that the degree of binocular central excitation, and accordingly of the tendency to movement, would be roughly proportional to the time during which a very brief stimulus was operative binocularly, and such a proportionality our experiments seem to indicate.

It may perhaps be suggested that this increase in stereoscopic relief with shortening time intervals was due, not to the particular reflex relations above mentioned, but to a general improvement of the conditions necessary for an apperception of a third dimensional kind. In other words that the relief seemed to grow greater as it actually grew clearer. We are not inclined to think that this is the real explanation, though of course the matter must await further explanation.

One other point has been emphasized in our own minds by these results and that is the rather misleading way in which binocular vision is often presented in the text-books. The subject is a complex one and much allowance is to be made, but still it is customary to talk of binocular vision as though it were due to an actual psycho-physical fusion of two simultaneously present but independent monocular images; whereas, it certainly should be regarded as the physiological result of a certain balance or distribution of excitations in a symmetrically constructed but unitary visual apparatus.

DISCUSSION.

The Method of Impression and Some Recent Criticism.

By E. B. Titchener.

In the course of a recent paper entitled *Die Grundformen der Gefühle*,[1] N. Alechsieff takes occasion to criticise the method of impression in general and my own work with that method in particular. I am not here concerned to offer a counter-criticism of Alechsieff's method and results; but I desire, so far as possible, to meet the objections which he urges against the method of impression.

I read with some surprise the following sentence: "In einer sehr eingehender Kritik hat M. Kelchner die Mängel der Eindrucksmethode hervorgehoben."[2] A second reading of Kelchner's article showed, however, that the statement has a certain justification. At the same time, it is distinctly misleading.

Kelchner[3] is recommending the use of the expressive method in connection with introspection. "Man hat," she says, "der Ausdrucksmethode gelegentlich vorgeworfen dass sie die Selbstbeobachtung vernachlässige." As a matter of fact it may be shown "dass die Selbstbeobachtung durch die Registrierung der Ausdrucksvorgänge in hohem Masse unterstützt werden kann." On the other hand, "ob *reine* Selbstbeobachtung imstande ist, den an sie gestellten Anforderungen zu genügen, muss bezweifelt werden." What Kelchner is criticising, so far, is *pure* introspection (italics K.'s), that is, introspection under casual, non-experimental conditions. The difficulties of pure introspection are two: unaccustomed direction of attention, and the fusion of affective processes with organic sensations.

It is clear that this pure introspection is not identical with the method of impression. Kelchner now proceeds to criticise, not the introspective value of that method, but the value of the introspections accompanying the method which she herself employed, the method of expression. There are three sources of error: liability to distraction of attention, with resulting scrappiness of observation; illusions due to the rules laid down for observation (*e. g.*, illusions of expectation); and inaccuracy of report. Kelchner seeks to show how and to what extent these errors may be combated in work done by the expressive method.

Alechsieff, however, goes on: "Sie findet dass diese Methode (the method of impression) alle Nachteile der reinen Beobachtung besitzt, und vor allem" the three just mentioned. That is to say, the three sources of error discovered by Kelchner "im Laufe unserer Untersuchung" by the expressive method are transferred bodily by Alechsieff to the charge of the method of impression, which Kelchner does not mention. I think that Alechsieff's statement may fairly be characterized as misleading.

There is, nevertheless, a certain justification for the statement.

[1] Wundt's Psychologische Studien, iii, 1907, 156 ff.
[2] *Op. cit.*, 162.
[3] Archiv. f. d. gesammte Psychol., v, 1905, 107 ff.

Kelchner concludes her criticism by saying: "Unsere Erfahrungen . . . tun . . . die völlige Unzulänglichkeit einer Untersuchung dar, die lediglich die Angaben der Vp. verwendet;" and she refers in a footnote to Orth's "Gefühl und Bewusstseinslage." When we remember that Orth's thesis, the work of which was done with Külpe at Würzburg, was accepted in 1903 by Meumann as a Zurich doctorate dissertation; and that Kelchner's investigation, made under Meumann's direction and edited by him, was ready for the printer in Feb., 1904: it is a safe guess that Kelchner had Orth in mind throughout her criticism of introspection. In so far, therefore, as Orth employed the method of impression, Alechsieff's statement may be constructively justified.

The method used by Orth is the method of question and answer[1] which Wundt, in a somewhat different connection, has quite recently criticised.[2] I am inclined to think that Wundt's criticism goes too far.[3] And I suppose that, in a very general sense, the method of question and answer may be described as a method of impression. At the same time, what one ordinarily thinks of, when the method of impression is mentioned, is either the serial method or the method of paired comparisons:[4] so that Alechsieff's statement is, again, misleading. Now the method of impression, in this customary and narrower sense, is not open to Kelchner's objections. Since a long series of stimuli, or of stimulus pairs, is laid before the observer, there is every chance for a constant direction of attention. Since the introspection required is of the simplest kind, there need be no scrappiness of observation. Since the rules laid down for observation are of the same unequivocal sort, there can be no illusion of expectation, etc. Since the 'report' consists simply in writing down a number or a letter, or in pointing with the finger to the one of two impressions, the chances of inaccuracy are minimal. On the other hand, the method has definite advantages. It allows us to cover a very wide range of stimuli, within a given sense department, in a comparatively short time; it furnishes an easy way of testing the constancy of the affective judgment whether for different observers or for the same observer at different times; and as the affective experience is cumulative, all of the same order, it affords excellent opportunity, in the intervals between sectional series, for detailed introspective analysis. Moreover, it is the only method now in the field which holds out hope of a differentiation, a determination of the nature and number, of the affective qualities. And lastly, it combines objective and subjective control, as every experimental procedure must do, and effects this combination more simply and more reliably than does the method of expression. I return to this point later. In the meantime it is enough to point out, first, that the introspective difficulties which Kelchner finds are, admittedly and evidently, difficulties characteristic of the method of expression and of that alone; and secondly that a method which, as Kelchner and Alechsieff put it, "lediglich sich auf die Angaben der Beobachter stützt," cannot be identified outright with the method of impression.

I turn to Alechsieff's criticism of my paper in the Wundt *Festschrift*. The critic's first point is that I am to blame for not having taken part in the experiments, "obschon sie nur auf Grund der subjektiven Methode gemacht worden sind."[5] This objection seems to rest upon a sheer misunderstanding. I explain in the paper itself that the work

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[1] The method really ranges from Brahn's Reizmethode (Philos. Studien, xviii, 1903, 133) to the Ausfragemethode proper; but there is no difference in principle.
[2] Psychol. Studien, iii, 1907, 301 ff.
[3] *Cf.* my remarks on Brahn, Philos. Studien, xx, 1902, 404.
[4] *Cf.* Wundt, Phys. Psych., ii, 1902, 267. The Reizmethode is not mentioned.
[5] *Op. cit.*, 167 f.

began with experiments of my own, in which I learned two things: that the required affective judgments might be passed without especial difficulty, and that introspection was facilitated by a particular phrasing of the instructions.[1] Alechsieff has apparently overlooked this paragraph. I then say that I took no further part in the experiments "als Versuchsperson," but that they were carried out "unter meiner directen Aufsicht." These statements mean simply what they say,—that I did not join the research groups, as observer, during the course of the published experiments: there is nothing in them to indicate that I discontinued my observations altogether. Indeed, I later promise for the future "eine exacte Verwerthung der Urtheilsdaten,"[2] which implies with sufficient clearness that I had gathered such data at first hand. Suppose that I had taken my place before the harmonical with the other observers: would not the critic have raised the cry of suggestion? Suppose that I had published my own curves: would not the critic have charged prepossession?

In his further criticism, Alechsieff confines himself to the objective aspect of my experiments, and says nothing of their subjective side. "Der Grundgedanke Titcheners ist der, dass es unmöglich sei, durch ein und denselben Reiz in derselben Weise zwei verschiedene Gefühlsqualitäten hervorzurufen. Dieser Gedankengang ist nicht zwingend."[3] But then it is not either my *Grundgedanke*. I quote from Hayes: "On the objective side there is the appeal to the 'curves.' . . . It would surely be a strange thing if a given set of stimuli affected a given observer by way of excitement-depression (or strain-relaxation) precisely as it affected him by way of pleasantness-unpleasantness. Coincidence might occur here and there; but the wider the range of observers, the larger the number of stimuli employed, and the more varied the type of the affective judgment, the less likely would it be, on the basis of the plural theory, that coincidence should appear. On the subjective side, again, there is the appeal to the introspection of the observers. If the observers declare that the affective judgment in terms of pleasantness-unpleasantness is direct, easy and natural, while judgment in terms of strain-relaxation and excitement-depression is forced, difficult, associatively mediated, etc., then the evidence of the method is in favor of the dual theory. And if, further, the observers state that their judgments of excitement-depression and strain-relaxation, so far as they are affective at all, are based upon pleasantness-unpleasantness, this evidence is proportionately strengthened. . . . The more numerous the observers, the more varied the stimuli, the more nearly exhaustive the affective categories, the more certain will the outcome be."[4] The method affords a twofold control, objective and subjective, and appeal to the subjective aspect is made with all plainness in my paper.[5] Alechsieff has overlooked these passages. He has also, publishing in August, 1907, overlooked altogether the work of Hayes, published in July, 1906.

I have no wish to deny that the method of impression is still in the first stages of its development. On the contrary, I believe that this characterization holds both of the serial method, in its two forms, and of the method of paired comparisons; still more of Brahn's stimulus

[1] Philos. Studien, xx, 1902, 388.

[2] *Op. cit.*, 390. Materials towards the analysis of the affective judgment have since been published from the Cornell Laboratory by Hayes (this *Journal*, xvii, 1906. 358 ff.); and further work, in continuation of Hayes' and my own, is now in progress.

[3] *Op. cit.*, 208; *cf.* 219. In the latter passage Alechsieff outlines a programme of further work, which I should have done well to undertake. This is always a safe line of criticism; but it would have come with better grace from a critic who had gone to the bottom of the work already done.

[4] *Op cit.*, 360 f.

[5] *Op cit.*, 389, 394, 394-6, 403, 405.

method, and its sub-forms of stimulus comparison and stimulus com pensation. We must not forget, however, that the method of impression, in any form, has received less extended trial than has the rival method of expression. There is now a tendency to combine the latter method with an introspective procedure, and we shall know before long within what limits the combination is possible. At all events, it is to the impressive method, or to what may be termed the impressive aspect of the combined method, that we must look for an answer to the question of the nature and number of the affective qualities. I hope, therefore, that in meeting Alechsieff's criticisms I have done more than merely remove a few personal misunderstandings.

PSYCHOLOGICAL LITERATURE.

Measurements of Twins, by EDWARD L. THORNDIKE. Archives of Philosophy, Psychology and Scientific Methods, No. 1, Sept., 1905. New York, Science Press. pp. 64.

In this monograph Prof. Thorndike discusses by modern statistical methods the results of a number of mental and physical measurements made upon fifty pairs of twin children between nine and fifteen years of age. The mental tests turned upon the finding and marking of A's distributed among other capital letters; the finding and marking of words containing two designated letters (e. g. *e* and *r*) distributed among other words; the finding and marking of misspelled words in easy prose; adding; multiplying; and the writing of words opposed in meaning to words in a given list. The physical measurements included height sitting and standing, length, width and circumference of the head, length of certain finger joints and of the forearm from elbow to finger tip, besides notes as to resemblance in general appearance and in color of eyes and hair.

The results are discussed with reference to the degree of mutual resemblance of twins in comparison with that of other siblings (*i. e.*, children having the same father and mother), the degree of resemblance of the younger twins as compared with that of the older ones, the degree of resemblance in traits assumed to be little subject to training as compared with those in which training is assumed to be influential, and the degree of resemblance in mental traits as compared with the resemblance in physical traits.

The conclusions reached are: (1) that the mutual resemblance of twins in mental traits is about twice as great as that of other siblings (about .80 as against .40—unity being the standard of complete identity); (2) that the older twins show no closer resemblance than the younger; (3) that there is not much greater resemblance between twins in traits assumed to be much influenced by training than in others; and (4) that the resemblances in physical and mental traits are of about equal amount. In general "the form of distribution of twin resemblances seems to be that of a fact with a central tendency at about .80 and with great variability restricted towards the upper end by the physiological limit of complete identity."

In addition to these explicit results Prof. Thorndike draws from the figures and his interpretation of them certain inferences with reference to general questions of heredity, as, for example, that heredity is a much more important factor in determining the relative attainments of human beings than is environment; "that heredity is itself highly specialized, each minute feature of physical and mental make-up possessing its representative in the germs and varying more or less independently of other features of the same germ;" and that twins are probably derived from two ova, not from a single divided one.

The statistical method is a potent and delicate instrument and all English speaking psychologists remain in Prof. Thorndike's debt for his efforts, of which this paper is a part, to make modern statistical methods current in psychology; but it is still a machine, and as such incapable of changing the intrinsic character of the raw data submitted

to it and of interpreting the final figures which it produces. With reference to the mental tests Prof. Thorndike is himself careful to say that he does not regard the particular tests as an adequate measure of mental resemblances in general and that the conclusions reached have reference only to the traits tested. This should, of course, be borne in mind in estimating the justification and weight of the general inferences above mentioned. That Prof. Thorndike himself seems sometimes to forget this and to underestimate the intricacy of the action and reaction of heredity and environment is perhaps the chief criticism to be brought against the paper.

Full tabular statements of the original data in the case of the twins and of the various stages of the statistical evaluation are given; but so far as the reviewer has discovered, this is not the case for the siblings with whom the twins are compared. E. C. S.

Sociological Papers, III. By G. A. REID, W. MCDOUGALL, J. L. TAYLER, J. A. THOMSON, P. GEDDES, A. E. CRAWLEY, R. M. WENLEY, W. H. BEVERIDGE, G. de WESSELITSKY, MRS. S. WEBB, and H. G. WELLS. Macmillan & Co., Ltd., London, 1907. pp. xi, 382.

Like its predecessor (this *Journal*, xvii, 1906, 429), the new volume of the Sociological Society's publications contains eleven original papers, accompanied by discussion, written communications, and the author's reply. The high level of the two previous volumes is fully maintained. Indeed, it is probably safe to say that there is no annual volume which offers a greater interest to philosophically inclined students of the sciences of life and mind.

The eugenic problem is approached from two sides, practical and theoretical. In his paper on A Practical Eugenic Suggestion, Mr. McDougall advocates the remuneration of the services of every person belonging to a specially selected class (*e. g.*, the class of civil servants) not, as at present, according to some rigid scale, but according to a sliding scale such that his income shall be larger in proportion to the number of his living offspring. From the theoretical side, Dr. Reid endeavors, in The Biological Foundations of Sociology, to throw light on the questions of human heredity and variability; and Dr. Tayler, in The Study of Individuals (Individuology) and Their Natural Groupings (Sociology), advances the thesis that the fundamental social formations are determined by the native characteristics of individuals.

A paper of great general importance to students of sociology is Professor Thomson's essay on The Sociological Appeal to Biology. The writer discusses various borderland problems involved in the relation of the two sciences, and thus places in their right connection a number of previously isolated studies. Professor Geddes's third paper on Civics (A Suggested Plan for a Civic Museum or Civic Exhibition and its Associated Studies) continues his attempt to afford insight into the life processes of the city.

A division of sociology not hitherto represented in the present series of publications, that of religion, is opened by Mr. Crawley's paper on The Origin and Function of Religion. Religion is defined as a psychic tone, temper, or diathesis; its sphere is the consecration of such elemental concerns as birth, adolescence, marriage, sickness and death; its objective, in one word, is life; and its first and last biological result is to raise human nature to a higher power.

Sociology is applied to the interpretation of concrete social phenomena in three papers: those of Professor Wenley on Sociology as an Academic Subject, by Mr. Beveridge on the Problem of the Unemployed, and by Mr. de Wesselitsky on the Russian Revolution.

Finally, two methodological papers (Mrs. Webb, on Methods of Investigation, and Mr. Wells, on The So-called Science of Sociology) deal with matters of practical procedure in sociological investigation.

M. W. WISEMAN.

American Philosophy. The Early Schools, by I. WOODBRIDGE RILEY. Dodd, Mead & Co., New York, 1907. pp. 595.

This work attempts to give a thorough and consecutive account of philosophy as developed in the United States from the landing of the Pilgrims to the advent of Emerson. It is based upon individual investigations, many rare works and unpublished manuscripts, and presents an account of the most important speculative movements, as they were transferred from Europe and developed during two centuries, thus slowly preparing the way for Emerson. It is the result of a three years' tenure of the Johnson Scholarship at Johns Hopkins. The chief sections are Puritanism, Idealism, Deism, Materialism and Realism. The present volume does not come down much beyond 1850.

Die Kultur der Gegenwart, ihre Entwicklung und ihre Ziele, herausgegeben von PAUL HINNEBERG. *Teil I, Abteilung VI, Systematische Philosophie.* Tuebner, Berlin and Leipzig, 1907. pp. viii, 432.

This volume is one of a series intended to present in encyclopedic fashion the whole circuit of modern intellectual life both theoretical and practical. Of the total work six volumes have appeared besides the present one upon Systematic Philosophy, for which the editor has been able to call to his assistance the ablest of German scholars. A work with a table of contents like the following is its own strongest commendation: DILTHEY, The Nature of Philosophy; RIEHL, Logic and Epistemology; WUNDT, Metaphysics; OSTWALD, Philosophy of Nature; EBBINGHAUS, Psychology; EUCKEN, Philosophy of History; PAULSEN, Ethics; MÜNCH, Pedagogy; LIPPS, Æsthetics; PAULSEN, The Future of Philosophy.

E. C. S.

Sammelbericht über die neuere Forschung in der Gedächtnis- und Assoziationspsychologie aus den Jahren, 1903-4, by HENRY J. WATT. Off-print from the Arch. f. d. ges. Psychol., 1906, 7 (Lit.), 1-48.

The author passes under critical review the more important papers in a bibliography of some sixty titles upon memory and the associative processes published during the years 1903-1904. The papers are treated in thirteen sections upon the following topics: General questions, General effects of practice, Methods and Instruments, Economical learning, Retention (perseveration), Influence of the feelings on memory, Recognition, Processes of thought, Association, Mediate association, Reaction times, Visual ideas, Characteristic mental types. The work appears to have been carefully and judiciously done and the review can be recommended as an excellent guide to the literature of the period which it covers.

E. C. S.

Leitfaden der Psychologie, von THEODOR LIPPS. Zweite, völlig umgearbeitete Auflage. Leipzig, Wilhelm Engelman, 1906. pp. 360.

Vom Fühlen, Wollen und Denken, Versuch einer Theorie des Willens, von THEODOR LIPPS. Zweite, völlig umgearbeitete Auflage. Leipzig, Johann Ambrosius Barth, 1907. pp. 275.

Both of these second editions fully justify their designation as *völlig umgearbeitete*. The first has not been greatly increased in bulk, but gives evidence at least in the table of contents of a thorough working

over of almost every section. The second is only nominally a reproduction of the earlier work of the same name, the author himself speaking of it as practically a new book with a special purpose indicated by its subtitle; "an attempt at a theory of the will." The number of pages have been increased by nearly two-fifths.

Both works are characterized by Lipps's singularly lucid style and by their lack of explicit reference to the work and views of others. History and controversy are wholly omitted and the infrequent footnotes refer almost without exception not to the work of others, but to sections in other works of the author where the matters in hand are more fully dealt with. Very much may be said in favor of such an unencumbered presentation of the subject matter, especially in works written for the general reader, though in the case of psychology it is apt to give rather too simple and unified an impression of the *status praesens* of the science. The omission of the brief introductory note, which in the first edition of the *Leitfaden* explained this feature of the work, makes the author's attitude especially liable to misconception by those who see the second edition only. E. C. S.

A Primer of Psychology and Mental Disease: for use in Training-Schools for Attendants and Nurses, and in Medical Classes, and as a ready reference for the Practitioner. By C. B. Burr. F. A. Davis Co., Philadelphia, 1906. pp. viii, 183. Price $1.25.

This little work falls into four parts. Pt. i (Psychology) gives a 40-page sketch of a faculty psychology, under the heads of thinking, feeling and volition, which might easily be improved as psychology, though it may do service as introducing the class of readers to which the book appeals to the more special study of insanity. Pt. ii (Insanity: 90 pp.) is a competent essay, from the medical point of view, on the definition, causes and forms of insanity. Pts. iii and iv outline briefly (15 and 25 pp., respectively) the method of treatment of cases from the medical and the nursing standpoint: they are sensibly and judiciously written.

The fact that the book, published in 1898, is now in its third edition is sufficient evidence of its practical utility. It would be well if the author could see his way, in the event of a future edition, to co-operation with some psychologist of standing for a re-writing of Pt. i.

P. E. Winter.

Anatomy of the Brain and Spinal Cord with special reference to mechanism and Function. For students and practitioners. By Harris E. Santee. Fourth ed. Revised and enlarged. P. Blakiston's Sons & Company, Philadelphia, 1907. pp. 453.

The author attempts to set forth the present status of the anatomy of the human brain and spinal cord, gleaning his facts from many sources. Since it is designed as a text-book, the subject matter is presented in an order convenient to the dissector and the descriptions presented from gross structures to the constituent neurons in each region. Embryology is used only where it assists in the comprehension of adult forms—this in the text, but a special chapter is also given to embryology. The special end in view is the localization of functional centres and the traits of their afferent, associative and efferent connections. Much stress is laid on origin, termination, course and function of conduction paths, and the more important and better known of these are summed up in the final chapter. Everywhere function is correlated with structure, and the function of each neuron is given in connection with its anatomical description. This, we should say, is the unique and most valuable feature of the book. The B. N. A. nomenclature is used almost without exception, the English equivalents of the Latin

terms being very largely employed. The student is expected to discuss as he goes along, and each should receive a section corresponding to those which are made units of treatment in this book.

Compensatory Motions and the Semi-circular Canals, by BENJAMIN C. GRUENBERG. Reprinted from the Journal of Experimental Zoölogy. Vol. IV, No. 3, Baltimore, Sept., 1907. pp. 447-467.

These experiments show apparent contradiction between the various responses of the frog to rotation on the turn-table and the theory of mechanical stimulation as the origin of these responses. A re-examination of the compensatory movements under these conditions shows the presence of a mechanical factor, the "spin," the significance of which in this connection seems not to have been considered before. From all the data the writer concludes that the compensatory movements of the frog's head caused by rotation arise in response to two distinct sets of stimuli: the visual, which is relatively feebler and slower, and the dynamic factor located in the internal ear, the latter involving rotation.

Cradle Tales of Hinduism. By the SISTER NIVEDITA (M. E. NOBLE.) Longmans, Green & Co., London, 1907. pp. xv, 343.

The stories here told form "a collection of genuine Indian nursery-tales," taken mainly from the Puranas, works which correspond to our apocryphal Gospels; from the Mahabharata, the Indian national saga; and from the Ramayana, the epic of Indian womanhood. The tales are entitled the Cycle of Snake Tales; The Story of Siva, the Great God; The Cycle of Indian Wifehood; The Cycle of the Ramayana; The Cycle of Krishna; Tales of the Devotees; A Cycle of Great Kings; and A Cycle from the Mahabharata. The reviewer is not competent to pass a technical judgment upon the authenticity of the stories themselves, or upon the historical questions raised by the author in her preface. He has, however, read the book with great interest, and can testify to the sustained excellence of the presentation and the intrinsic value of the tales. H. W. HOTCHKISS.

Hygiene of Nerves and Mind in Health and Disease, by AUGUST FOREL. Authorized translation from the second Berlin edition by Herbert Austin Aikins. G. P. Putnam's Sons, New York, 1907. pp. 343.

This interesting book well merited translation and that of Professor Aikins is lucid and conscientious. It is divided into three parts; the first treating of mind, brain and nerves in their normal condition. This is an admirable compend for what is now known, is sufficiently illustrated for the author's purpose, and constitutes a little more than one-third of the book. The second part is on the pathology of the nervous life, containing the general idea of pathology, a synopsis of mental diseases and abnormalities and their causes. The third part discusses the hygiene of mental life and of the nervous system. Professor Forel is never dull and his discussions abound with concrete cases and applications which give an unusual liveliness to his pages.

BOOK NOTES.

Concerning the Intelligence of Raccoons, by L. W. COLE. Reprinted from the Journal of Comparative Neurology and Psychology, May, 1907. Vol. XVII, No. 3. pp. 211-261.

The author has tried to determine what type of association the raccoon is able to form, the complexity and the permanency of its associations, and to ascertain whether mental images and the tendency to imitate are present in this animal. The original plan was to make observations upon the senses, instincts and habits of the raccoon in general, and to compare these observations with those made upon other mammals under similar experimental conditions. These observations, however, are omitted from this article. All were based upon six young raccoons—four males and two females. They learned to release fastenings, to discriminate form and color (when associated with brightness), to imitate, showed a good deal of power to learn from being put through an act; and the reactions to the present and mental images were marked. The coon, the author thinks, stands midway in the rapidity of the associations it forms between the monkey and the cat, being nearer the former in their complexity. Only after practice are motor associations permanent, and two types of learning and forgetting are clearly distinguished. The coon discriminates form, size, tone, and color when combined with intensity. There is no evidence that the coon imitates its fellows. Many of Mr. Cole's conclusions are confirmed by similar observations by Mr. H. B. Davis, published in the last number of the *American Journal*.

The Dancing Mouse, a study in animal behavior. By ROBERT M. YERKES. The Animal Behavior Series, Vol. I. The Macmillan Company, New York, 1907. pp. 290.

The writer treats the origin and history, feeding, breeding and development of the young and then turns to the special discussion of the behavior of the dancing mouse, equilibrium, dizziness, structural peculiarities, sense of hearing, sight, color, educability, etc. He also conducts the young mice to the labyrinth, tests their discrimination and the efficiency of various training methods, studies duration of habit, memory and relearning, individual, age and sex differences, inheritance of forms of behavior. It is an interesting and painstaking study and all those who care for this kind of work will await with interest later volumes of this series.

The Measurement of Variable Quantities, by FRANZ BOAS. Archives of Philosophy, Psychology and Scientific Methods. No. 5, June, 1906. New York, the Science Press. pp. 52.

This short monograph gives the substance of a course of lectures given by Prof. Boas for a number of years to students of anthropology, biology, and psychology at Columbia. Because of the small mathematical attainments of such students as a class, the treatment for the most part steers clear of the calculus. The reviewer with others is grateful to Dr. Boas for thus making his lectures generally accessible, but conscience compels him to say that since Dr. Boas has not been able to eliminate the natural difficulties and complexities of the sub-

ject along with the calculus, the non-mathematical student will not find the monograph easy reading. E. C. S.

Poetry and the Individual, an Analysis of the Imaginative Life in Relation to the Creative Spirit in Man and Nature, by HARTLEY BURR ALEXANDER. G. P. Putnam's Sons. New York and London, 1906. pp. x: 240.

The general character of this work can perhaps be most briefly indicated by saying that it is a modern work on the æsthetics of poetry conceived and executed in the spirit of Plato, and possessing much the same sort of excellencies and defects that characterize that master. The author says in his preface that the reason modern idealism is deficient in vital effectiveness is that its representatives have taken up the "cultivation of intellectual subtleties to the neglect of the practical idealism which their thought should really contain." "The philosophical need, then, is *humanisation* of philosophical interests. . . . . The book here offered aims by a sort of natural criticism to lay bare some of the instinctive modes of human thought and to assist the major philosophical task." The work is literary in manner—at times over much so—an essay always, rather than a scientific treatise,—evidently the work of a man of insight who has dealt at first hand with that of which he writes. E. C. S.

Twenty-fifth Annual Report of the Bureau of American Ethnology to the Secretary of the Smithsonian Institution 1903-4. Government Printing Office, Washington, 1907. pp. 296, and 129 plates.

This is a very timely study of the aborigines of Porto Rico and neighboring islands by J. N. Fewkes, who also appends a paper on certain antiquities of Mexico. Both are copiously illustrated and, indeed, about half the volume is made up of full-page cuts besides those inserted in the text.

The Physician's Visiting List for 1908, published by P. Blakiston's Son and Co., Philadelphia. Price $1.00.

A conveniently arranged physician's diary, address book and cash account, containing also other data likely to be useful to a forgetful man of medicine. E. C. S.

The Lords of Ghostland. A history of the idol. By EDGAR SALTUS. Mitchel Kennerley, New York, 1907. pp. 215.

This work consists of chapters on Brahma, Ormuzd, Armon-Râ, Bel-Marduk, Jehovah, Zeus, Jupiter, Ne Plus Ultra.

Normal Activity of the White Rat at Different Ages, by JAMES ROLLIN SLONAKER. Reprinted from the Journal of Comparative Neurology and Psychology. July, 1907, Vol. XVII, No. 4. pp. 342-359.

Menschen- und Tierseele, von E. WASMANN. J. P. Bachem, Köln, 1907. pp. 16.

The New Mysticism. Six lectures given in Kensington, and at Cobham, Surrey, November, 1906, by ADELA CURTIS. Curtis & Davison, London, 1906. pp. 196.

Philosophical Problems in the Light of Vital Organization, by EDMUND MONTGOMERY. G. P. Putnam's Sons, New York and London, 1907. pp. 462.

Shake-Speare England's Ulysses, by LATHAM DAVIS. G. E. Stechert & Co., New York.

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A GLANCE AT THE PHYLETIC BACKGROUND OF GENETIC PSYCHOLOGY.

By G. STANLEY HALL

The background of the genetic aspect of everything is time, which is as indefinable and as primordial as its counterpart, space. There is nothing really actual or conceivably possible that is either spaceless or timeless, and to try to think anything so is stultification. Force, atoms, electrons, souls, thoughts, feelings, and all other activities have position, extent and duration, though we may not be able to measure them. Nothing can be outside of, before, after or independent of either time or space. If the universe were resolved into ether or nothingness, or to die Clausius's heat-death, or to vanish by the slow evaporation of all its solid matters, these would remain unaffected, for they are independent of their content as they are of knowledge. Both are continuous and unbroken, vaster than any imaginable bounds, yet indefinitely divisible. Or if infinity mean only that process is endless, there is nothing transfinitive in the sense of the older theories. They have been thought to be empirical and *a priori*, objective and subjective, have been ascribed to sense, reason, memory, will and special and general feelings, and faculties have been invented as their organs,[1] they are, in fact, more or less involved in every psychic activity, animal or human, concrete or abstract, but their a priority is not logical but genetic, for they are the

[1] For general history of views upon the subject, see Joh. Jul. Baumann Die Lehren von Raum, Zeit. u. Mathematik in der neueren Philosophie, 2 Bde., G. Reimer, Berlin, 1868; also H. Nichols: The Psychology of Time. Clark Univ. Thesis. Henry Holt & Co., New York, 1891.

only truly metaphysical objects. Their universality is not because they are ineluctable factors of thought, but conversely they are the latter because they are the former. Both have their poetry, their religion, also their illusions, phobias and panics. Instead of being the complex mysteries which epistemology and even the usually clear headed Wundt[1] makes them, nothing else is so simple or homogeneous. It is for this reason that knowledge becomes scientific just in proportion as it is exactly defined in their terms, because they are the best known of all things.

To teach them to be mere subjective forms, existent only for us in the sense of Kant,[2] which is oriental in spirit and scholastic in method, is, at the same time, the acme of man's anthropomorphism and conceit of his place in the universe, which in ancient Greece would invite the nemesis of the gods, and is today a crime without a name against the soul of youth just at the time when the passion for certain and objective reality is strongest and when vulnerability to such sophistication is greatest. The objectivity of time and space begins in the very ectoderm from which the nervous system is an infoldment, and to make even the present, which seems the most real part of both, merely specious, despite the fact that to focus everything remote into the magic here and now, is the best definition we have of genius and sanity, is to belie the whole phyletic experience of the soul which acquired through innumerable generations all that is innate in the individual and betrays a subtle mark of incipient mental decay. With the growth of mathematics and a dozen other sciences, our conceptions of time and space, both small and great, have in late years been extended by leaps and bounds, so that the old philosophical ideas of them are far transcended, while psychology has developed rich, fruitful and complex special fields for each. They remain indefinable because by them and by mass all things else are explained.[3]

Time is, at bottom, duration and motion is its measure. Though we can conceive no beginning or end, it is not, therefore, necessarily infinite or eternal, and to ask whether every flitting instant which takes from the future and adds to the past makes the one shorter and the other longer is a vain query. Time contains every causal series that exists at once and

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[1] W. Wundt: Die Unendlichkeit der Welt. Essays, 2te Aufl., Engelmann 1906, pp. 136-184.

[2] Henerle (Die Theorie der Materie) sees in matter only a necessary hypothesis and in the new theories of it only what might have been deduced from the old atomism.

[3] See general conception of space. Adolescence, Vol. 2, pp. 159 and 540. D. Appleton & Co., New York, 1904.

might contain innumerable others. Every effect follows its cause in time, short though the interval be. All histories are forms of construing it. It is irreversible and would go on in the same direction if at the end of some great Platonic era the horology of the temporal order and sequence of all things were inverted and the old grew young and evolution became involution. It would have no gap if all thought and all change stopped for a time and then went on again. It is neither a construction nor an entity. It is not all form or content, concrete or abstract. We cannot affirm that it is finite or infinite, but it defies and transcends all these distinctions as it baffles every definition, but just is, a sun-clear fact, the *primum movens* of all genetic psychology. A recent subtle philosopher[1] makes it a chronic disturber of speculative systems, ever creeping in and making all unstable, and defines time as a negation, as "abstract or dynamic non-being," and the latter as "the genus within which time is the species." If we understand this weird conclusion it is the most flagrant metaphysical perversion of the plain truth about time in a long sad history of them. We must, on the contrary, regard the highest and most complete knowledge or science of anything as the full and accurate description and explanation of all its developmental stages in their temporal sequence. When we can answer the question what occurs at each step, and why, there is no more left to know. Evolution thus, gives a new ideal to, and a higher standard of, noetic values which transcend many lower types, such as definition by genus and difference, classification, deduction from categories, analysis that is introspective, but without perspective. The subordination of these older static methods is in the interests of positive and progressive truth.

Our life is brief and so absorbed with what is near that we are mentally myopic for time. This almost constitutes a new fallacy or idol which impels us to explain all things by their nearest cause, partial though it be, as if this were a law of logical parsimony instead of mere sluggish inertia. Just as it was easier to say that fossil shells on the Alps were dropped there by Crusaders, who we know did carry some as charms, than it was to develop the science of palæontology, so it is simpler to say that the morbid fears of children are due to early frights, and their anger, pity, water, tree and sky-psychoses to their own infant experience, to the exclusion of palæo-atavistic influences. Some regard time as if it were a limited bank deposit so that our economy must be severe and we must admit only grudgingly every new demand lest our account be overdrawn.

[1] J. E. Boodin: Time and Reality. Monograph Supplement of the Psychological Review, October, 1904. Vol. VI, No. 3, 119 p.

Difficulties in the expansion of our ideas of space, though somewhat analogous, were earlier and far more easily overcome. The provincial mind is too prone to regard product rather than process, and finds it hard to realize that everything is hoary with age, that all things we know are in flux, are terminals of a vast past and germinals of a vast future, and that the highest science is the law of change. It is an inveterate habit of thought inherited from ages of superstition that makes us prone, when confronted by seeming breaks or chasms in nature, to fly to some hypothesis of supernal intervention or creationism. The catastrophism psychosis, chronically to prefer unproven revolution to yet unverified evolution, is only a moderate improvement upon miracles. Some appear to feel that they hold a brief either for religion or pedagogy to minimize time, and so urge with almost partisan zeal that the period of authentic history and culture was brief, that man was not tertiary or life pre-Huronian, that the world is really new, etc., as if chronological concepts were in danger of hypertrophy, the sea room of thought restricted, or as if, like valley dwellers, they craved a limited horizon. The opposite tendency is now more scientific, truer, psychologically and pedagogically better, so that if in doubt, and assumption is needful, we should choose the longer and not the shorter time, not merely to indulge the momentum of evolutionary thought, but as an aid to clearer insight and to larger views of the universe.

If all the bodies of our solar system were resolved into nebulæ and uniformly diffused through the sphere of which Neptune's orbit would be a circumference, or if all the matter of the 1000 million suns and yet more dark bodies in our sky were thus diffused, they would hardly dim the passage of the light of a faint star, so infinitesimal is matter compared to the space through which it is distributed. And yet some geologists are now inferring from the structure of the earth, and some astronomers from the composition of meteorites and other heavenly bodies, that, slowly as systems were evolved and rare as collisions are, all celestial bodies have grown up and been resolved back to all stages of chaos by collisions of every kind, degree and angle, perhaps many times, and that the most constant orbits are products of selection of safest paths, so that the entire history of our earth from cosmic gas serves only as unity in computing that of the universe. In the solar system we hear of an ancestral sun that preceded ours, and thus meteors, hundreds of which strike our earth daily, though in one billion years they would add but one inch to its surface they have, nevertheless, in one form of the planetesimal theory, built it up. Indeed, the structure of some of these wandering bodies suggests a destructive and reconstructive history as indubitable as

that of metamorphic rocks. On this view the present visible universe is only one of the countless forms which its substance and energy have taken on, and the oldest objects in any corner of it are novelties to a mind vast and ancient enough to grasp the larger history of its eternal flux. Still more perhaps in the abyss of time, all possible combinations of the innumerable elements of the cosmos may have occurred. Of course all this is utterly unproven, but if we need time beyond the power of the higher arithmetic to compute, we may take it freely.

Hutton[1] thought that terrestrial history showed no trace of a beginning and no prospect of an end, and Lyell[2] despised cosmogony and would have geology accept nothing from astronomy or physics. Only with the rise of evolutionary thought did the problem of the age of the world acquire vital interest.

Suess[3] says that while we can use interstellar distances as unities to aid us in conceiving astronomic spaces, we have no apparatus for geologic time. The age that separates us from common fossils, or perhaps two of them from each other, is, like those celestial bodies, without parallax, which inform us of their physical constitution by their spectrum, but furnish no clue of their distance.

George Howard Darwin[4] holds that the moon broke away from the earth at least 56,000,000 years ago, and, perhaps, much earlier, and that when it did so, charged with steam and gas, pressure diminished as it receded and it fairly boiled with explosions and volcanoes. This, of course, constituted one great epoch in the history of our globe. A second "consistentior status" was when the earth grew solid at a surface temperature of 1200 C°, which Lord Kelvin[5] placed between 20 and 40,000,000 years ago. The next critical period was when the temperature fell to 370 C° and steam became water, a stage which Joly[6] puts between 80 and 90,000,000 years ago. Geikie[7] would be contented with the 100,000,000 years for the whole process. Although the interior of the earth was well on to

[1] James Hutton: The Theory of the Earth. Trans. of the Royal Soc. of Edinburgh. Edinburgh, 1785.

[2] Sir Chas. Lyell: Geological Evidence of the Antiquity of Man. J. Murray, London, 1873.

[3] Edward Suess: The Face of the Earth, tr. by Hertha B. C. Sollas. Clarendon Press, Oxford, 1904-1906. Vol. II, pp. 5, 56.

[4] The Evolution of Satellites. Smithsonian Inst. An. Rep., 1897. Govt. Printing Office, Washington, 1898.

[5] William Thomson Kelvin: The Age of the Earth as an Abode fitted for Life. Smithsonian Inst. Ann. Rep., 1897, pp. 337-357. Washington, 1898.

[6] John Joly: An Estimate of the Geological Age of the Earth. Smithsonian Inst. Ann. Rep., 1899, pp. 247-288. Washington, 1901.

[7] Sir Archibald Geikie: Geological Change and Time. Smithsonian Ann. Rep., 1892, pp. 111-131. Washington, 1893.

solidification, it slowly yielded to great pressure like Barus's diabase, so that as the water sought and found the lowest level the ocean base sank still more and the land parts of the surface were proportionately raised. If with Sollas we assume the total maximums sedimentary deposits to be 50 miles thick, man now lives at the top of 34 miles of vertebrate fossils. Sollas[1] seeks from very many different data on which estimation can be calculated to assign the term of years to each geological age, assuming all the strata together to have a total thickness of 265,000 feet, and the rate of accumulation to be a foot per century, his total time is 26,500,000 years.

To come down to recent ages, estimates of time since life first appeared on earth have been often made by astronomers, physicists, geologists and palæontologists. The bases for induction in these fields differ and hence the results are very divergent, ranging from a few score thousand to hundreds of millions of years. Very different and quite as difficult and conjectural are the attempts often made by geologists to assign absolute or even relative duration to the different geological ages. H. Schmidt and Haeckel present the relation which they think occupied by each of the five geologic evolutionary periods by taking one hundred million years as the age of life and reducing it to one creation day of twenty-four hours. In this case the archæozoic period, occupying 52 million years, would be represented by 12 h. 30 m.; the palæozoic period, to which a duration of 34 million years is assigned, would be 8 h. 7 m.; the mesozoic age (11 million years) would equal 2 h. 38 m.; the cenozoic (3 million years) equal 43 m.; the anthropozoic period (140 thousand years) would equal 2 m.; the historic period (6 thousand years) 5 s.; the Christian era (2 thousand years) between 2 and 3 s.[2] Our individual life is so short that several generations of men would have to summate their exact determinations to prove that the minute hand of a clock, measuring thus the cosmic day, moved at all. The conclusion that it was not stationary would seem dangerous and fantastic to ephemera whose lives endured only a second, however intelligent they might be.

A pupil of Haeckel[3] on a somewhat different basis has actually ventured to estimate the number of generations since vertebrate life began. He begins by assuming that 250 generations at 20 years each would carry us back to 3000 B. C. The *pithecanthropus* is thought by some to have lived near the

[1] William J. Sollas: The Age of the Earth and other geographical studies. T. F. Unwin, London, 1905.

[2] See Ernst Haeckel's Last Words on Evolution. 2nd ed. A. and C. Black, London, 1899. Tr. from the 2nd ed. by Joseph McCabe.

[3] Ernst Haeckel: The Last Link. Appendix by Hans Gadow. A. Owen & Co., London, 1906. p. 120.

beginning of our last glacial epoch. If we place this 270,000 years ago and assume that this forbear of man attained puberty at 16 or 20, then some 17,000 generations would lie between him and the lowest human tribes. According to Croll[1] and Wallace, 850,000 years take us into the Miocene, with the *pliopithecus* and the *dryopithecus*. If they lived, then, 600,000 years before the *pithecanthropus* and were pubescent at ten, we cannot get less than 60,000 generations. From the apes to the lowest lemurs in the lower Eocene, assuming five years for puberty, we need 420,000 generations. If we go back to the *prototheria*, the earliest known mammals in the Triassic, we must add the whole of the Jural and Cretaceous stage, or in all 5,500,000 years, and assuming three years per generation we get 1,800,000 of these. The whole stretch from the lowest fish to man would be thus resolved into more than five million generations. each of which would mean only a small step. If we admit 17,000 generations between the man of to-day and the *pithecanthropus*, the change in each generation would be too slight to perceive or conceive. So in the change from the fish to the amphibian, if there are one million stages, the marvel rather is that so many generations should be needed to bring about the result. The stretch of time from the lowest fish to the beginning of life is probably vastly greater and the generations certainly succeeded each other far more rapidly.

Taking all the strata where they are thickest, we have a total vertical depth of perhaps twelve to fifteen miles above the simplest Silurian vertebrates, and below this stretch two Palæozoic and five Proterozoic ages and the Archæan complex, together probably quite as thick. Chamberlain and Salisbury[2] think that on the new accretion hypothesis "the real beginning of life on the earth greatly antedated even the oldest accessible formations, so that fossil evidence will never solve the problem of the origin of life." Still these authors prefer the term "archeozoic" to "archean," not because they believe in any form of the Eozoon hypothesis of traces of life in the Laurentian, which in the years 1863-94 was so hotly debated,[3] but because convinced that although metamorphism and other changes have obliterated every trace of it, life originated during this age. Of the geological record as a whole, Darwin said, "I look at the geological record as a history of the world im-

[1] James Croll: Stellar Evolution and its Relations to Time. D. Appleton & Co., New York, 1889.

[2] See T. C. Chamberlain and R. D. Salisbury: Geology. H. Holt & Co., New York, 1906. Vol. 2, pp. 160 and 276.

[3] See G. P. Merrill: Contributions to the History of American Geology. U. S. National Museum Rep., 1904, Part II, Chap. IX, p. 635. Washington, 1906.

perfectly kept and written in a changing dialect. Of this history we possess the last volume alone, relating to only two or three countries. Of this volume only here and there a short chapter has been preserved, and of each page only here and there a few lines. Each word of the slowly changing language, more or less different in successive chapters, may represent the forms of life which are entombed in our consecutive formations and which falsely appear to us to have been abruptly introduced. On this view the difficulties above discussed are greatly diminished or even disappear."[1] This opinion and Darwin's avowed agnosticism concerning the origin of life seem to Haeckel faint-hearted, and in view of the new light which phylogeny and ontogeny now shed upon each other, Darwin would perhaps have modified both views to-day.

The simplest and most generalized types of life doubtless came first, but we can only conjecture its forms before the hard parts, preserved in fossils, arose. Even the trilobites, which abounded in the Cambrian, show marked developmental stages which must have recapitulated a long phyletic evolution of the species. From the very oldest and simplest petrified forms of life to uni-cellular organisms, is a long and intricate way, but now richly set in scene by both classification and embryology. Modern biologists are so infatuated with the marvels of the cell and its parts that they have neglected the stages by which the simplest bit of structureless chromacea or moneron-like bit of protoplasm evolved into the cell. The origin of the former, which is true archigony, is still another problem occupying a vast period of which we know still less.

When we launch out from the farthest shore of microscopic visibility the vast uncharted ocean of the infinitely little stretches before us. The strongest microscope can hardly see the 100-thousandth of an inch, yet a molecule of hydrogen, consisting of two atoms, has a diameter of about one 250,000-millionth of an inch. About one thousand atoms is now the estimate in a highly organized vital molecule, yet we know very definitely that it takes about a thousand electrons to compose a single atom of hydrogen. The green glow inside an airless Crookes's tube, which is due to incandescence set up by a stream of electrons which may be deflected by a magnet as if it were a piece of iron, is caused by corpuscles which can be counted and their charge measured. The rate of leakage of charged bodies surrounded by gases which varies with their pressure, temperature, with light and moisture, have given phenomena which have led within the last few years to epoch-making results con-

[1] Charles Darwin: Origin of Species. 6th edition, D. Appleton & Co., New York, 1904. Vol. 2, p. 88.

cerning the ultimate constitution of matter.[1] The corpuscle and the charge it carries are each known only through the other. The electron is in some sense a mental image, and while it may be a strain or a whirl in the ether, the conservative view is that the corpuscular theory is favored and that Ostwald's energetics, the way to which was paved by Willard Gibbs's[2] views of energy and entropy, are very extreme. The recent studies of radium mark perhaps, the climax of human ingenuity in research. Like every other, the radium atom consists of a whirling mass of particles, some charged with positive and others with negative electricity. From this, probably on account of centrifugal force, some of these charged particles fly off with intense velocity. In passing through a gas they dash to pieces such of its component molecules as lie in their line of flight. So great is the atomic weight and the initial energy of each projectile that it can destroy some hundred thousand molecules before its velocity is reduced 40 per cent. Thereafter its power to break up or ionize the gas molecules declines rapidly. At least seven transformations of the original atom have been traced from radium A to F, and the close relations of these forms of radium to uranium, thorium and perhaps to lead, suggest the transmutation of matter. The loss of each particle causes a change in the properties of what is left behind, which differ greatly as do the different rays, the velocities of which are very distinct, though nearly constant for the same set. When they cause phosphorescence, they seem to do so only by re-enforcing the pre-existing molecular activity of the elements in the substance itself. Aggregates or clusters of these electrified corpuscles, absolutely identical in themselves, probably built up the more than fourscore chemical elements by variations in their number and arrangement—that is, they are *materia prima*. Thus light, heat, electricity, perhaps gravity and chemical affinity are forms of radio-activity, and volcanic force and even life itself are thought by some to be due to it. In the beginning was radium.

It is little wonder that so much energy with so little matter should give an impulse to dynamism, but the saner thought is that these units are more than centres of force and that, small as they are, they have a sub-stratum or core of solid matter. At least they act very like tangible bodies, moving in straight lines, if not deflected, and if they are so, the radii of their curves

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[1] See J. J. Thompson: Conduction of Electricity through Gases. Cambridge University Press, 1903. A. Righi: The Modern Theory of Physical Phenomena, Macmillan, 1905. E. Rutherford: Radio-Activity. Cambridge University Press, 2nd ed., 1905.

[2] J. Willard Gibbs: Thermodynamische Studien aus dem Englisch übers von W. Ostwald. Engelmann, Leipzig, 1892.

can be calculated by the same formulæ as apply to the movements of the heavenly bodies. When they are arrested their energy is transformed into heat exactly as when a hammer strikes an anvil, and if they penetrate an atom they lose energy at a rate proportional to the square root of the atom's weight. Still these epoch-making discoveries do make us feel that the world is intensely alive or that its inorganic basis is no less but perhaps more vividly active than life itself, so that if now the soul were thought material it would seem less degraded by its origin, and its resolution into such a sub-stratum would be less repugnant. Even therapeutics has realized new relations between these forms of physical energy and life, and theories of energetics do not oppose but favor the psychic interpretation of the world. All these processes preceded and will outlast life. They are valid in every part of the universe so that their beginning is inscrutable.

Among the chemical elements, carbon, which predominates in nearly every part of plant and animal life, is marked by the number and variety of its compounds. Of these sixty thousand have already been isolated and studied, and yet, although Shenstone[1] estimates that it makes up about half of modern chemistry, the science of this element is still in its infancy. Its fecundity is seen by the fact that it has been estimated that no less than 802 tridecanes may perhaps exist, each of which would have distinct properties and yet could not be distinguished by chemical analysis the one from the other, since each has the same proportions of carbon and hydrogen and the same chemical formula. When it is added that but very few of these 802 substances are found among the sixty thousand compounds known, even the "marvels of radium pale before the possibilities that lie hidden in a handful of soot or charcoal," and we can, perhaps, understand the inspiration of the hundreds of men who for the last fourscore years have devoted their lives to the study of this element alone. The clue to this labyrinth was furnished by isomerism, which designates those cases where several distinct compounds arise from uniting the same elements in the same proportions so that properties of compounds do not depend on the nature and number of atoms in their molecules alone, but also upon the way in which they are arranged. Now there are hundreds of isomerisms and the existence and the properties of undiscovered compounds can often be predicted with a degree of accuracy hardly inferior to that which enabled Mendeléeff by his periodic law to predict

[1] W. A. Shenstone. New Physics and Chemistry. Smith, London, 1906.

the existence and foretell the properties of gallium and germanium.[1]

Another line of work which has loosened the hard-trod soil about the problems of life and mind consists in the exploration of close analogues of vital phenomena in inanimate things. Poets and philosophers have always thought there was a soul in things. Kepler was an animist and thought that the motive force of the planets was their soul. To him, almost as to the ancient hylozoists, the globe was a great animal, sensitive to astral influences, frightened into hurricanes and earthquakes by the approach of other planets. Leibnitz' continuity theory held that there was no inorganic kingdom and nothing quite dead, but a perfect and unbroken continuity, so that every, even a material, monad had a rudiment of both life and soul. Boscowich[2] did not refuse to his immaterial and infinitely small points a kind of low vitality. The alchemists and Hermetic philosophers went yet further. In point of fact, too, everything in nature works. Some rocks and precious stones are spoken of as more vital than others. A metallic rod stretched shows eventually a weak point where it would break, but if it is given a little time to rally, this threatened point is hardened, and when stretching is resumed another weak point is developed. Metallic alloys have a mobile structure. Hammering and torsion have a consecutive effect, almost like after-images. Mercury sweats through iron. Copper is welded to tin by pressure. Glass slowly accommodates to torsion.[3] Wax so hard as to be scarcely indented by the thumb nail, placed in a hole above a cork, with pebbles on it, in a few days shows the cork on the top and the pebbles at the bottom, both having passed through the wax as if it were fluid. If a cylinder of lead be placed on a disk of gold and kept in boiling water, which is far below the melting temperature of both these metals, in six weeks shows the gold diffused on the top of the lead cylinder. When annealing breaks down a crystal form the molecular displacement finds a new equilibrium. Bose[4] found that tin and other metallic wire, after the passage of an electric current, required various periods of recovery, and that there were even analogies to tetanus, both complete and incomplete. Some wires show

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[1] D. Mendeléeff: The Principles of Chemistry, translated from the Russian, 6th ed., by George Kamensby. Longmans, Green & Co., London, 1897. pp. 26-90-124.

[2] William Thomson, Lord Kelvin: On Boscowich's Theory. Smithsonian Inst., An. Rep., 1889. Washington, 1890. pp. 435-439.

[3] For a collection of similar instances see Albert Dastre: Life of Matter. An. Report of the Smithsonian Institute, Washington, 1902-1903, p. 393.

[4] Jagadis Chunder Bose: The Response of Inorganic Matter to Stimulus. Royal Institution Lecture. W. Clowes & Sons, London, 1901.

fatigue analogous to that of nerves if acted upon for some days. Some chemical substances stimulate and others depress and poison action in both wires and nerves. Bose even constructed an artificial metallic retina, responding to color and with oscillations after the cessation of light, not unlike those shown in the retina. Many other phenomena raise anew the question where to draw the line between physical and physiological processes, as if life activities were foreshadowed in things without life, and above even this great distinction there were a larger unity in and through all. Not only memory but hysteria have been used to designate the behavior of bodies subjected to magnetic and other forces. Again, the Brownian movements, seen in microscopic dust in a liquid which is suspended in water, never cease, for they are found in quartz crystals which at the moment of their formation enclose a cavity of water containing a bubble of gas. Only in 1894 was it more or less explained by Gouy as an oscillation of independent particles, the larger moving slowly and the smaller most active. They are not vital because seen in boiled liquids, and the movement does not depend upon the nature or form of the particles or of the liquid unless it is viscous. They are independent of the tremors of the earth and seem to be molecular movements that invite us to study the far more subtle ones of the kinetic theory.

Crystallization approaches most nearly to life. Crystals grow from the surface by apposition rather than, like germs, from within. They assimilate by a process that mimics nutrition, and may attain a great size. If the mother lye is removed their development is suspended like a seed kept from soil and moisture. Each tends to carry out its own architectural plan and heal wounds, restoring mutilations more rapidly where they occur than it increases at other points. Ismorphism, or their power to replace each other, is comparable to inbreeding or crossing, which is the touchstone of taxonomic relationship. Crystallization is very closely associated with simpler vital organisms in their hard parts, bone and shell. Something like reproduction occurs and crystals are sown like micro-organisms. Liquids in suffusion are especially favorable media for propagating certain kinds of crystals and contact of such a fluid with any crystal germ by an object not sterilized sets up at once the process of crystalline organization which spreads through the mass. In Ostwald's salol the crystals may measure less than one one-hundredth of a millimeter on each side and in hyposulphate of soda they measure a thousandth of a millimeter. Sometimes they are spontaneous generations where the optimum conditions of a solution occur. Often the latter cannot be reproduced and so crystals can be generated only by

infection or filiation. Perhaps the most striking instance of this is the famous glycerine which crystallized spontaneously in 1867 in a tun sent from Vienna to London in the winter. No one saw these crystallize and the conditions under which they occur were entirely unknown until recent years. They have been also accidentally formed in a French factory, and only, so far as known, in these two cases. They appear just as living forms do in a favorable environment and spread only as the Promethean fire did by direct contagion of flame or coal, or as magnets were made only by contact with other magnets before electro-magnetism. The crystals of 1867 have already a very extensive posterity. One factory produces them on a large scale. As they melt at a temperature of 18° C., a single summer might for a time exterminate the whole species.

Most matter is not amorphous but crystalline, and snowflakes, sand, rocks, minerals and most solids in solution tend to take on forms of marvellous intricacy, beauty and mathematical regularity, which are very diverse and of characteristic structure. This used to be explained as a *nisus formativus* of nature. The analogy between the framework of plants (which inclined Sachs[1] to the view that plant protoplasm is at bottom crystalline), as well as the skeletal forms of many lower animal forms and crystals, is very suggestive and has long been provocative of speculation; and now that crystallography is experimental, this morphologic principle seems increasingly life like. The physicist Lehmann[2] has summed up our knowledge of the structure of doubly refracting colloids, many of which strikingly suggest cells, fibres, and other biological patterns, karyokinetic figures, etc. While admitting that protoplasm has a structure that both expresses and directs molecular force, he discredits all strictly vital functions. Schenck[3] supplemented this work from the standpoint of a physical chemist with quantitative measurements of anisotropic substances. These studies show that besides solid crystals there are manifold others of various degrees of plasticity and fluidity in colloids, the consistency of which resembles the softer and most vital parts of living substance. Von Schrön[4] less temperately concludes, from a study

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[1] Julius von Sachs: Vorlesungen uber Pflanzen-physiologie. Engelmann, Leipzig, 1887.

[2] Otto Lehmann: Flüssige Kristalle. (With many photographs on 39 quarto pages.) W. Engelmann, Leipzig, 1904. Also his Theorien des Lebens.

[3] Rudolf Schenck: Kristallinische Flüssigkeiten und flüssige Kristalle. W. Englemann, Leipzig, 1908. 1905.

[4] Biologia Minerale. Lettera del Prof. Otto von Schrön al Prof. G. B. Milesi. Estratto dalla "Rivista di Filosofia e scienze affini" Ottobre, 1901. Anno. III, Vol. V. N. 4. Zamorani e Albertazzi, Bologna,

of what he calls the growth, behavior and generation of crystals, that everything in nature either lives or has lived. All young crystals, but not old and fossilized ones, are alive. His greatly magnified photographs show, he thinks, that "petroblasts" or bioblasts pass through certain stages before they are usually called crystals, which latter term designates a degenerate though perdurable stage. Here, too, probably belong Leduc's[1] so-called artificial cells of potassium, ferrocyanide and gelatine, and also Dubois's vacuolids, or, as he now calls them, eobes, produced by barium and manganese salts in sterilized boullion. Burke's[2] now much debated radiobes, so named to chime with microbes, arise when particles of radium are sprinkled on jelly-like decoctions of nutritive substance rendered antiseptic. These structures seem to develop nuclei, to grow by physical metabolism, bifurcate, adjust inner to outer relations, etc. Though aggregates of crystals, they are not, we are told, exactly crystals nor colloids in disguise, but add to this the most primitive elements of vitality. Although "on the bounds between crystalline and organic bodies, they cannot properly be called living, but correspond to some simpler form of life that existed in a distant age."[3]

Not only crystals, but certain foams, resemble cells or tissues. Quincke[4] found in solutions of silicic acid, glue, etc., after evaporation, fibres, fissures, tubes, bubbles and often vacuoles which are either open or closed, join each other or do not, according to the viscosity of the oily liquid. They seem to grow and shrink by diffusion in water. The inclination of their walls, surface-tension and refraction also change with the concentration of the medium. He thought them cellular, yet

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1901. Also Le due Conferenze dimostrative ed una Còmunicazione fatte a Napoli al Congresso contra la Tuberculose nel 1900, dal Prof. Otto von Schrön. Estratto dagli Atti del Congresso. R. Pesole, PiazzaB ellini n. 6, Napoli, and Brevi cenni sulla conferenzo dimostrativa su materia e forza (uno dei capitole della vita dei cristalli). C. Sciarrino, Palermo, 1906.

[1] Stephane Leduc: Cytogenise experimentale. Ajaccio, Paris, 1901.

[2] J. Butler Burke: The Origin of Life; Its Physical Basis and Definition. Chapman and Hall, London, 1906.

[3] These studies should tend to rescue from discredit R. Altmann's attempt (Die Elementarorganismen und ihre Beziehungen zu den Zellen, Veit & Co., 1894), to resolve protoplasm into ultimate granules which he called bioplasts and thought to be ultimate vital units essentially crystalloid in nature. These were within the range of the microscope, regularly disposed in viscous intergranular substance, and, though differing in form, were essentially homogeneous. His mistake was in describing as granules various components of the cell known to be both different and secondary.

[4] G. Quincke: Ueber periodische Ausbreitungen Flüssigkeitsoberflächen und dadurch hervorgerufene Bewegungserscheinungen. Annal. der Physik u. Chem., Neue Folge, Bd. 35, No. 12, 1888. p. 580-642.

soluble. Bütschli [1] studied for years emulsions of soluble salts and thinks the structure of photoplasm is like fine soapsuds or beer, crowded and flattened like the bottom of honeycomb cells. Under the microscope these structures show spontaneous flowing movements, explicable by physical principles. Foams have since been much studied as a new avenue of approaching the mysteries of life. By others, such alveolar structures are interpreted not as globules, but as fibrillar, like the threads constituting a sponge. Flemming, especially, thought primitive plasm to be filar and showed that such network structure, the fine threads of which are sometimes isolated and sometimes bundled together, which play an important rôle in cell division and are prominent in large ganglionic cells, is common in cells and can be more or less imitated by coagulations. They are, however, probably a secondary phyletic product. Enzymes and catalysors which set up processes in substances, themselves remaining unchanged, although they shed valuable light upon vital processes, cannot explain the origin of life.

Yet more valuable and stimulating is the recent effort to explain the elementary phenomena of life and its developmental processes by the general principles of physics and chemistry.[2] Capillarity accounts for the sphericity of cells as of dewdrops from diffusion over surfaces. Contact, adhesion, solubility and positive chemotropism are the key to primitive food-taking as absorption is of rudimentary digestion, and excretion of chemical repulsion. The appearance and disappearance of vacuoles are osmoses. The marvellous radilarian framework is due to the mechanism of fluid crystals. Artificial substances perform some, if not most, of the characteristic amœboid movements which are always toward points of least and from those of greatest surface tension which is ever changing under the influence of temperature and many inner and outer influences. Ciliary motion is reduced to metabolic disturbances of myeline threads. Roux's [3] reduction of bifurcation to positive and negative cytotropism, the mechanical imitation of spindles,

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[1] J. A. O. Butschli: Untersuchungen über mikroscop. Schäume u. das Protoplasma. W. Engelmann, Leipzig, 1892.

[2] Represented in the Arch. f. Entwickelungs-Mechanik der Organismen, founded in 1889. See in this Archiv. Ludwig Rhumbler: Physikal. Analyse in der Lebens erscheingungen in der Zelle. Oct., 1898, Bd. 7, pp. 103-350, 100 figures. See, too, his Zellen-Mechanik im Zellenleben. J. A. Barth, Leipzig, 1904. Various articles in Ergebnisse der Anatomie und Entwickelungsgeschichte during the last 10 years. Also Dr. Paul Jensen, Untersuchungen über Protoplasmamechanik. Pflüger's Archiv. Nov., 1901, Bd. 87, s. 361-417.

[3] Wilhelm Roux: Der Kampf der Theile im Organismus. W. Englemann, Leipzig, 1881. Ueber die Bedeutung der Kerntheilungsfiguren. W. Engelman, Leipzig, 1883.

rays, cell division, gastrulation and other cytokinetic or embryological processes, Boveri's[1] interpretation of the nucleus as a storehouse of energy and matter, the most-of-all-discussed karyokinetic figures partly reproduced artificially by Bütschli, illustrate this field. The best of this vigorous group of investigators by no means attempt an ultimate explanation of the nature and origins of life, although very outspoken against the neo-vitalists, but they do show that many of its first and simplest manifestations are physical and chemical processes and are best described in the terms of these sciences. Certainly points, currents, gravity, light, heat and all the great cosmic forces to which living forms respond by the many tropisms, tonuses, taxies, kineses, etc., suggest that the rapport between life and its environment, although at first closer than now, is still very intimate.

The above attempts to derive life do not altogether explain the reverse of the universal process of death even in the simplest organisms. Were this done, the origin of life would be resolved into that of matter itself, and the curious question suggested by Roux would arise—into what species, plasm made in the laboratory and without heredity, would evolve, and, we may add, what kind of soul stuff these *eobionta* would have. Celluloid crystals, foams, surface tensions, etc., do suggest that nature may have made countless abortive attempts to produce life, that this was her longest and hardest task, occupying all the vast Archæozoic age, that many methods that we can now partly reproduce were eliminated by selection, and that many more of them culminated in the development of a matrix of physical conditions under which the vital spark was at last struck, and the process of developing plasms may have been going on from the first. Very likely the chasm between all these artifacts and life is so great that all the first and longest geologic age was necessary to bridge it. It is not impossible that at its advent life, instead of being akin to the lowest forms now known, was very different and is now a missing link.

Three other notable theories of the origin of life are the following: In 1865, following a suggestion of Liebig's, Hermann Eberhard Richter, reviving and definitizing old speculations, proposed the theory that germs of lower organisms, detached from rapidly moving celestial bodies, were floating through space, and that life was inseminated on this earth by them. There was thus an interplanetary exchange of germs, and wherever these found a stage of development with warmth, moisture, etc., favorable to life, they adjusted themselves with

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[1] Theodor Boveri: Ergebnisse über die Constitution der chromatischen Substanz des Zellkerns. G. Fischer, Jena, 1904.

great plasticity to new environments. Assuming that somewhere in the world, planetary bodies had always existed with life upon them, the latter was assumed to be eternal, and the problem was how these cosmozoa were transported from one world to another. Traces of carbon and petroleum-like substances and, indeed, something akin to humus, are often detected on meteorites. These germs, he assumed, might live a long time in great desiccation and without food, like the spores of micro-organisms in a condition of apparent death. Helmholtz[1] and Sir William Thompson[2] have commented not unfavorably upon this possibility. Not only do meteorites have carbon compounds, but the spectrum of the light emitted from the heads of comets suggests gases containing carbo-hydrates. In passing so rapidly through our atmosphere only the surface of large meteorites is heated. In view of all the failures to demonstrate abiogenesis, Helmholtz thought it a fully justified scientific process to inquire whether life be not thus as old as matter.

Perhaps the boldest theory is that of Preyer[3] (1880) that living substances are primary, and lifeless material is a secondary secretion from it. He assumes that we must emancipate ourselves from the arbitrary and factitious idea that life can exist only on a protoplasmic basis. Primevally the whole substance of this earth was a giant organism of fiery fluid or gaseous matter. The entire movements of and within it were its life. As, however, it cooled, substances like the heavy metals solidified or died and step by step ceased to take part in the life of the whole, and the first dead inorganic masses were formed. As cold progressed and the surface of the earth grew rigid or dead, and the chemical elements were differentiated from what remained, combinations resulting in protoplasm became possible.

Pflüger's startling theory[4] now incorporated into the scheme of Haeckel,[5] assuming that plasma is due to the properties of albumen, first points out the marked difference that while living albumen can decompose itself, dead albumen, as, *e. g.*, in the white of an egg, maintains its integrity for some time.

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[1] H. von Helmholtz: Popular Scientific Lectures, trans. by E. Atkinson. Longmans, Green & Co., New York, 1903. p. 196.

[2] Sir W. Thompson: Report of the British Association for the Advancement of Science, 1871. pp. lxxxv-cv.

[3] W. Preyer: Naturwissenschaftliche Thatsachen und Probleme. Paetel, Berlin, 1880, and Ueber die Erforschung des Lebens, Mauke, Jena, 1873.

[4] E. F. W. Pflüger: Physiologische Verbrennung in Lebendingen Organismen. 1875.

[5] Ernst Haeckel: The Wonders of Life. Harper and Brothers, New York, 1905. p. 345.

This instability of the former is due to the intra-molecular oxygen stored up which dissociates its complex molecules and forms new groups, *e. g.*, water of its hydrogen and carbonic acid of its carbon. In their non-nitrogenous elements the decomposition products of living differ a little from those of dead albumen. But the nitrogenous products of living far exceed and differ radically from those of dead albumen in producing creatin, the nuclein bases, guanine, etc., all of which as a marked characteristic either contain cyanogen (composed of one atom of carbon and one of nitrogen as a radical) or else like uric acid can be made out of its compounds. Hence Pflüger[1] infers that living albumen always contains cyanogen and dead does not. Now living albumen and cyanic acid are both transparent at low temperatures, but set and darken with heat, while with water both break up into water and ammonia and both produce urea, the first organic substance to be artificially composed by Wöhler[2] (1828). Both have great power to incorporate into their molecules other like components and to grow catenally or by chains. Hence Pflüger concludes that cyanogen is a half living molecule and that in it life begins. The first albumen was alive. A constant molecular weight is not necessary for these monstrous molecules, in size like the sun compared to small meteors, and which are incessantly growing and diminishing. Hence the question of the origin of life resolves itself, according to this chemical train of reasoning, into that of the origin of cyanogen. We thus find ourselves confronting the remarkable fact that cyanogen and its compounds can only arise in intense heat, so that they may have originated while the earth was wholly or in part in a fiery condition. Moreover the other essential components of albumen, like the carbo-hydrates and the alcohol radicals, can arise synthetically at high temperatures. Hence life arose from fire, and the long ages during which the earth's surface has been cooling gave plenty of time for the many polymeric formations. The very easy decomposability of compounds into which this root of life enters, and its close relations to carbon compounds were maintained after water arose; and from the chemical relations with its dissolved salts and gases evolved the living albumen, and while they did not produce anything with the morphological value of cells, they enable us to trace the origin of protoplasm. Of course there was a long series of intermediary stages between the most developed Vulcanic pryozoic radical and the simplest living plasm, and Neumeister[3] and

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[1] E. F. W. Pflüger: Archiv. für Physiologie, 1875. Bd. 10, S. 251-367.
[2] F. Wöhler: Annalen der Physik und Chemie, 1829. Bd. 15, S. 525.
[3] R. Neumister: Betrachtungen über das Wesen der Lebenserschein ungen. G. Fischer, Jena, 1903.

other vitalists have urged that this chasm is impassable, although living albumen always does contain cyanide or products of its compounds. To the graver objection that such heat compounds would perish when water appeared, it can only be said that during the immense period yet remaining to be bridged after the closest approximation, both chemical conditions and processes which transcend the limits of even that great science, may have prevailed.

As to the size of the smallest bits of matter to which we can attribute life, several writers have made interesting estimates. Errera[1] estimates the limit of smallness of organism based upon a special study of one Micrococcus of the diameter of 0. 1 μ which, he estimates, contains something like 10,000 molecules of albuminoid substance and 3,000 atoms of sulphur. After making several such estimates he concludes that we may say with a degree of probability, which is of the same order as the probability of the molecular theory of matter, that there can exist no organisms which are to ordinary bacteria as these are to higher organisms. Thus there can be no living creatures hundreds of times smaller than those now known, so that the great spectacle of life unfolds within relatively narrow and well determined limits. McKendrick's[2] estimates are yet more interesting and exact. He thinks the smallest particle that can now be seen under the best microscope is about one 20-thousandth of a millimeter in diameter. That certain bacteria are smaller than this is shown by the fact that after porcelain filters strain out all that can be seen, the filtered liquids infect with certain diseases and kill as quickly and surely as do unfiltered cultures, indicating that it is the microbes themselves and not their toxines that are fatal. Weismann assumes the diameter of a molecule to be one 2-millionth of a millimeter, and that a biophore contains some 1,000 molecules. Its diameter would then be one 200-thousandth of a μ, or ten times too small to be seen. Thus a cube, one side of which was one 1-thousandth of a μ would contain 8 million biophores, and a red blood corpuscle between 3 and 4 thousand million of them. The smallest visible particle may contain 1,250 molecules, for there would be 125 in a biophore. The head of a spermatozooid, of which man produces 340,000 million during his sexual life, is estimated to contain 25,000 million, and the fecundated ovum 25,000,000 million organic molecules. Even if each molecule contained 10,000 atoms there would be 1,000 million of them. On this

[1] M. L. Errera: Sur la limite de petitesse des organismes. Rev. Scientifique. Feb. 7, 1903. 4e Série, Tome 19. p. 169-172.

[2] John G. McKendrick: Presidential Address. Report of the British Association for the Advancement of Science. Glasgow, 1901. pp. 808-816.

basis each human spermatozoon would contain 250 billion atoms or 250 billion elecluns. This would be only the contribution of the male to his offspring; the far larger ovum would contribute more. Hence, although there are estimated to be 60 trillion cells in the adult human body, there are enough elements in the germ plasm to account for the heredity of all the qualities which connect us with our forbears for countless generations, and to allow each individual to contribute something to all his posterities without drawing upon ulterior atoms or electrons with which life is probably in some unknown way continuous. While, like most biologists, Weismann, *e. g.*, thinks life demands a definite combination of different kinds of molecules and says, "A single molecule cannot live, can neither assimilate nor grow nor reproduce." Haeckel objects, for the plastidule he assumes is a single plasm molecule to which he ascribes not only these powers but memory in Hering's sense. Verworn's[1] biogens, of which plasm is composed, may also be single molecules which may or may not be homogeneous. The plasmogony they effect is not a mixture as Hertwig opines, nor does symbiosis of independent elements explain the nature of the higher units. They are rather like electrons, postulates for economic thinking, and their reality is closely analogous to that of God, freedom and.immortality for Kant's practical reason. Although most of them are metamicroscopic, they are not too small to play the marvellously complex rôles assigned them. The secrets of the origin of soul are now more and more clearly seen to be bound up, if not identical, with those of the origin of life, and the beginnings of both stretch back ever farther in time and down the scale of simplicity, so that their primordial germs must be coeval with the dawn of matter and with time itself. Although, as we know them in their present forms, they seem incommensurably different from the life of the physical universe, they are, in fact, products of an evolution that has proceeded by insensible gradations with no rupture of identity.

We next glance at the most characteristic of the more complex vital units, which in modern biology play a part psychologically very akin to that of categories and innate ideas for the philosophers of some generations ago. Nägeli[2] estimates that there are 100 billion of his micellæ in a single moneron of 0.6 m. in diameter. If one of the former has a diameter of 0.0006 m. it would still have millions of ultimate parts, but a single molecule cannot be said to live. The micellæ are not

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[1] Max Verworn: Die Biogenhypothese. G. Fischer, Jena, 1903.

[2] Karl Wilhelm von Nägeli: Mechan. physical, Theorie der Abstammungslehre. Oldenburg, München, 1884. Sections 1-5.

themselves living but are so composed as to give rise to life. We can never observe their origin, since their development from an inorganic basis is not an empirical datum, but a result of reasoning from the laws of matter and force. This primitive abiogenesis occurred many times in the past and still occurs, and may occur at any time and place, so that life is polygenetic. In general, the most evolved forms have perhaps the oldest pedigree. Simple forms that have remained at the lower stages of evolution may now be many times reproduced. The micellæ strongly tend to combine into chains and ropes in a crystalloid way, and this arrangement conditions all the later developmental differences of species in an almost fatalistic predetermining way not consistent with the plasticity demanded by broad selectionists. Variation is thus definitely directed and to a great extent independently of the conditions of the environment. His evolutionism is so extreme that morphology and taxonomy are called phylogenetic sciences, but all goes on from the momentum of an internal principle which he calls isagitation (from ἰσάζω, to make equal). Inherent in nature is an impulse to perfection, and over all his idioplasm is a kind of *nisus formativus*, which plays upon the rows or threads of micellæ like a pianist on keys. His earliest probionta, plassonella or young monera are not structureless organisms without organs, but are nearer to unicellular algæ, and over against the hereditary matter of idioplasm is the trophoplasm, which is the nutritive part of the cell. He thinks, as does Weismann, that life probably arose in a reticulated superficial layer of fine porous clay or sand, where the molecular forces of solids, fluids and gases could best co-operate. Under their influences these biophoridæ acquired the power of assimilation such as plants have, then of multiplication, and finally were able to cross the threshold of microscopic visibility, although, perhaps, after enormous periods of time, for before any advantages of differentiation could occur, biophores must have made stable associations into colonies. Nägeli finally thinks that if even molecules have anything like sensation it must be agreeable to them to follow and painful to depart from their attractions and repulsions. Hence, he assumes a spiritual or psychic bond throughout all nature, of which the human mind is only the highest development.

Haeckel's[1] plastidules are chemical molecules which can be

[1]Ernst Hackel: Natürliche Schöpfungsgeschichte. 1st ed., G. Reiner, Berlin, 1868. Systematische Phylogenie, G. Reiner, Berlin, 1894-06 3 v. See also The Last Link, tr. by Hans Gadow, 2nd ed., A. and C. Black, London, 1899. The Riddle of the Universe at the Close of the Nineteenth Century: tr. by J. McCabe, Harper and Brothers, 1901. The Wonders of Life: tr. by J. McCabe, Harper and Brothers, New York, 1905.

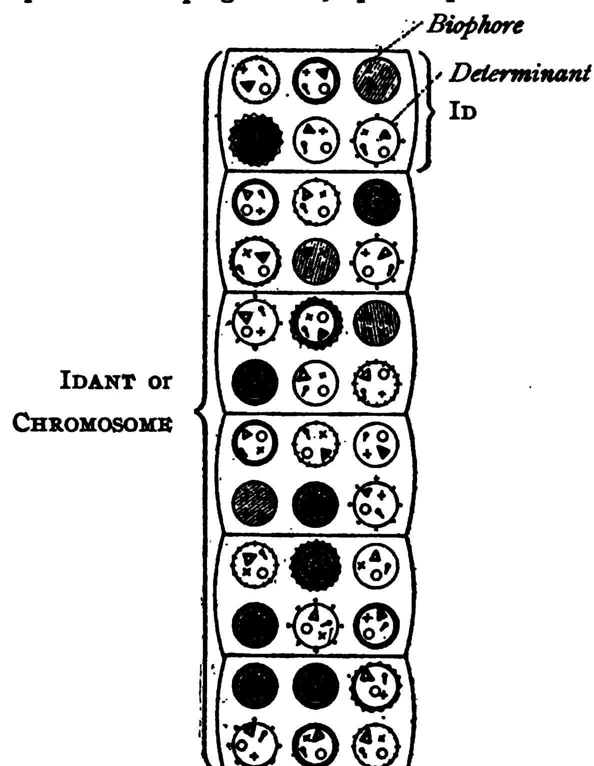
decomposed only into atoms. Hence they cannot divide but they can cause the production of new ones from a nutritive fluid. In a broad sense everything lives, even atoms, for their attractions and repulsions suggest pleasure and pain, but their will and sensations are invariable. In a stricter sense, life is reproduction and that is essentially memory, and this plastidules have, but lifeless things lack. Primordial memory is a mechanical result of the mode of movement of the elements of the blastidules. Their first organisms are monads or now chromacea which grow, as it were, by precipitation of new plastidules and then reproduce by division. At first, they are homogeneous and alike, but the environment adds new inner movements to those pre-existing and these adaptations are transmitted. This differentiation is perigenesis. Each new individual passes rapidly through the stages of its stirp, guided by the unconscious memory of the plastidule, which in higher organisms have both learned and forgotten much, but in lower ones have learned little and forgotten nothing. All the ontogenetic processes of one generation from egg to egg are comparable to a wave with smaller waves representing the successive cell divisions that form and develop the organs during growth, while the history of a species is a larger wave: but since environment changes, the crests and hollows of even these waves are not a straight line, but themselves make a larger wave curve representing the entire history of life. Perigenesis is the efficient cause of this complex and ramified undulatory reproduction of the plastidules. Life has one origin and Haeckel boldly constructs many elaborate genealogical trees for various phyletic groups of animals and plants, Man is given a very elaborate pedigree from the plastidule up. Acquired adaptations are inherited in a Lamarckian sense. He abhors teleology, has been himself a great ferment, stimulated many pupils, and has in recent years popularized his ideas in many papers and several books.

Weismann,[1] who, next to Darwin, is probably the ablest of biological thinkers, has since 1875, wrought out a theory of life that fits and unifies a vast body of facts, and in his last volumes gives a final formulations of most of his views. Life arose by chemical spontaneity long before the first fossils. The primitive *biophoridæ* were produced after countless failures and

[1]August Weismann: Essays upon Heredity and Kindred Biological Problems. Authorized trans. 2nd ed., Clarendon Press, Oxford. 2 v. 1891-92. Studies in the Theory of Descent, translated and edited by Raphael Meldola, S. Low, Marston, Searle and Rivington. 2 v. London, 1882. The Germ-Plasm, tr. by W. Newton Parker and Harriet Rönnfeldt. C. Scribner's Sons, New York, 1893. The Evolution Theory, 2 v., tr. with the author's co-operation by J. A. and M. K. Thomson, E. Arnold, London, 1904.

extinctions, but eventually with a wealth so inexhaustible that vastly more species than ever did evolve might have done so from them. This process, perhaps, occupied more time than all that has since elapsed. These biophores are the smallest units that can assimilate and reproduce, but they are extremely diverse. At first they aggregated, then organized in innumerable ways and relatively few of these products survived and only the fittest of them slowly advanced towards microscopic size. Even if we could compose the conditions for spontaneous generation, we should probably never know that it had occurred, so minute would be its first products. Everything that lives grows and divides so that there was no death at first because there is nothing like a corpse to be sloughed off. Hence, barring accident, the lower forms of life when once started were potentially immortal and from these most ancient beginnings all living forms are developed by a direct continuity of descent. Plasm is like a fluid poured over nature, preserving its every feature, so that their vital structure, large and small, has a definite cause in the environment, to trace which historically for every cell, organ, species and corm is our goal. No one has given such extension to the principle of selection which is germinal, histonal and personal. Each biophore, tissue and individual struggles to survive and in so doing competes for nutriment and space with every other in each individual body. There is ultimately no predetermination and every species is like a traveller and may depart indefiniteiy from its origin and wander about as the environment favors or opposes. When the cell arises the chromosomes contain both cytoplasm, which presides over ontogenic growth and nutrition, and also idioplasm or ancestral plasm, devoted to reproduction. In the former, cell division is differential and produces parts ever less general and more special. Germ plasm, however, divides identically. Instead of disintegrating to form a soma which dies, it is perpetuated indefinitely through successive generations now in the highest as it was originally in the lowest organisms. There may be at a certain critical stage a struggle between the two, and the ancestral elements may overcome and expel the histogenic plasm which makes the body and thus constitute a reproductive instead of a somatic cell. Biophores may be massed into determinants and one, at least, of these presides over every part of the body that can vary independently of other parts. Ids are groups of determinants which comprise all of the ancestral plasm necessary to build up an individual. Very early in the embryo the two kinds of plasm are separated, that for reproduction being set apart as latent for subsequent generations, ready to develop when their time comes. So isolated and protected is it that all the events in the life of the individual

barely affect it and hence acquired qualities are not inherited. To the vindication of this extreme position a wealth of concrete discussion is devoted. Higher yet are the idants which may contain a hundred different ancestral plasms and which is used to explain atavism. Though all these processes cannot be traced in detail, they, in fact, pass over definite tracts and through distinct stages. Even the parts and organs produced by the first divisions in ontogenesis, he seeks to trace as the cells slowly specialize and lose the reproductive power originally inherent in all. In a sense, development into classes, orders, families, genera, species and individuals is thus the disintegration of germ plasm as its progressively specific qualities come



This diagram (for Lotsy's Deszendenztheorien. Fischer, Jena, 1906) illustrates how, although the chromosomes divide equivalently, this does not involve equivalent diversion of the ids. Hence certain groups of determinants may be lacking in some cells, which would therefore have only a part of the hereditary qualities and so could no longer reproduce a new individual.

out. When maturity is attained each single cell has its directing biophore. Each vital element is intrinsically different and is not made so by its position or environment, as Hertwig's epigenesis holds. Germ plasm acquires more and more potentialities up the ascending stages of life, so that although it was at first very simple, it is now for the higher types extremely complex. Plain as this abstract of his theory seems, it becomes indefinitely complicated and requires various ancillary assumptions when he seeks to explain by it, as he does in great detail, the various functions of heredity, variation, regeneration, hybridism, alternation of generations, amphimixis, atavism, parthenogenesis, infection, and most of the other great problems of plant and animal life.[1]

De Vries[2] thinks that variations as now tabulated in curves, showing their range in the same species, are too small to produce the latter by gradual departure from the parent form. He also holds that crossing cannot produce permanent qualities that are also really new, but that, after breeding true for a series of generations, there comes a period of saltatory variation or mutation, and that this is the chief agent in producing new forms. Selection, thus, has a limited range and can only

[1] Mention should perhaps here be made in passing of what is in some respect the counterpart of Weismanism, recently developed by a few pathologists, Cohnheim, Jensen, and Beard which is rudely as follows. Alternation of generations means that at intervals an individual is produced which does not develop but stops at some lower ancestral stage, the next generation reverting to the full normal of the species. This is usually thought to occur only in very low forms but it is now believed that it holds in a changed way in metazoa and even man, both of which have larval structures which degenerate. The embryo proper develops on the larval bases which do not thus constitute new organisms but are the foundations on which development begins *de novo*. These transitory structures or embryonic residua constitute the trophoblast and at a critical period when the pancreas develops trypsin, they are digested as its alkaloids supplying the formic acid intra cellular digestion. This responsible issue has the same continuity as germ plasm, only it is as the cells, and these aberrant germ cells are found in all parts of the body of the young, arising outside of it and migrating into it. They wander anywhere along different paths, but some never reach their goal in the sex organs, and are misplaced but do not degenerate. The embryo is a product of one of the primary germ cells, while the rest are its twin brothers. Instead of producing trophoblastic tissue they may degenerate and sink to a low plane of rapid multiplication and lose the power of building tissue. Although these perverted germ cells tend to produce new individuals they are able only to produce larval, sexual trophoblastic tissues and these may produce a ferment called malignin which destroys the health cells of their host. This is cancer, which causes about one death out of forty.

[2] Hugo de Vries: Species and Varieties, their Origin by Mutation. The Open Court Pub. Co., Chicago, 1905. Intracellulare Pangenesis. Fischer, Jena, 1889.

explain the survival but not the arrival of the fittest. This view is favored by the gaps in the palæontological record. To the illustration of this thesis and its defense against his many critics, he has massed a vast area of facts from a wide biological field. His pangenes are the smallest chemical substances that can assimilate food, grow and divide, and although vast in numbers they are of limited kinds, so that just as the letters of the alphabet compose all words, they can make all living forms. In the simplest creatures all the pangenes are few and act concurrently, but in the higher forms of life they are more numerous and of more kinds. They may long rest quiescent and then mass themselves and surmate their activities in the molecular plasm. Here they multiply and each nucleus stores up all the pangenes of an individual, becoming thus the organ of heredity. From here they migrate at intervals and form new individuals. At first every cell can reproduce itself from the pangenes resulting from the first divisions which are alike. Soon for some this generative power lapses into that of growth only because the cells that compose the body do not receive a complete set of pangenes, for it would be useless to burden them with those for which they have no use. Before long, however, the somatic development becomes unique and apart. Pangenes are distributed not like Darwin's, by permeating all tissues, but always at the moment of division, so that their movements are always confined within the limits of the cell. They have their phylogeny like species.

J. Reinke[1] represents a totally different point of view. Like Lotze, he lays great stress upon the difference between mechanism and teleology, is a dualist and a theist. After pushing physical and chemical explanations to their uttermost, as he always tried to do, and insisting that every form of life is a chemical factory and pervaded by systems of force from the sun, which all structure is to conduct, he finds reproduction, nutrition and sensation to be inexplicable because they have no analogies in nature. Hence he is led to postulate teleological forces not unlike the entelechies of Driesch.[2] These he calls dominants or higher energies. They are not subject to the laws of matter, are metaphysical, unconscious, yet psychic. Each smallest vital part has its dominants and they give all regularity and are revealed to us by the changes they cause in structures. The latter they may find ready-made by earlier dominants. They are also higher dominants of species, and

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[1] Johannes Reinke: Die Welt als That. 4te Aufl. Gebr. Paetel, Berlin, 1905. Einleitung in die theoretische Biologie. Gebr. Paetel, Berlin, 1901, Philosophie der Botanik. Barth, Leipzig, 1905.

[2] Hans Driesch: Der Vitalismus als Geschichte und als Lehre. Barth, Leipzig, 1905.

soul is their sum in the individual. God did not create one primitive cell only as Wiegand thought, nor one for each stem, but they are all from, and closely related to, pervading cosmic intelligence. Weismann's germinal selection he calls the Indian summer of Darwinism, whose winter is at hand. As opposed to evolution, he thinks bacteria may be regressive forms, apes, degenerate men and apogamy may arise from sex. His view is in sympathetic rapport with the conscious teleology of August Pauley[1] and with the action according to a purpose without consciousness of a purpose which Hartman[2] pleads for in this field.

Cope[3] calls the organizing force which makes life out of dead matter bathmism and the first unit it produces a plastidule. Anagenesis is the way up and kinetogenesis is the development of organs by movements and tendencies to move. He holds, as does Orr, that habitual acts have a morphogenic function. Katagenesis is retrograde development. Some plants are degenerate protozoons. The first most elementary phenomenon of life is rudimentary consciousness which has attended, if it did not direct, every step, *e. g.*, the development of the heart and intestines and their movements. But every function strongly tends to lapse to automatism and mechanism which is more simple and stable. Thus unconscious memory is basal and ontogeny or diplogenesis is motivated by this fallen but potent memory of the stages of the development of the race. Life is thus energy directed by sensibility or by mechanism that it originated. The beginning was energy becoming conscious, and consciousness having performed its part lapses into habit, function and structure.

Zehnder,[4] starting from purely physical and chemical principles, thinks that the lowest and first life unities are molecules, arranged in tubes or fistellæ, from rings, of which there are millions in the plasm of every cell and which are specialized so that some effect endosmosis, some contraction, others conduction, etc. Each tends to produce another like it and so they double rapidly. They are of different sizes and complexities, Their vortical shape make them permeable for atoms and even molecules which pass through their centre, and this accounts for absorption, swelling and growth. They expand and con-

[1] August Pauley: Darwinismus und Lamarckismus. Reinhardt, München, 1905. Especially Chaps. XI and XII.

[2] Eduard Hartmann: Das Problem des Lebens. Haacke, Bad Sachsa, 1906.

[3] E. D. Cope: The Primary Factors of Organic Evolution. The Open Court Pub. Co., Chicago, 1896.

[4] Ludwig Zehnder: Die Entstehung des Lebens. J. C. B. Mohr, Tübingen, 1899; and Das Leben im Weltall. J. C. B. Mohr, Tübingen, 1904.

tract, but this oscillation is about the constant size and this is the *Anlage* of muscularity. They are, nevertheless, essentially crystalline. Arranged in rows, often with conical ends, they constitute cylinders, contain fluid, and form these simplest tissues, in a square millimeter of which there are about 1,000 milliard fistellæ. No other structure can give such stability. Conductivity once established, psychic life begins, the early evolution of which he traces in some detail. The stellar universe is a larger and analogous vital organism.

Hatschek[1] finds two elementary processes of life, both due to rhythmic or phasic constitutional changes of biomolecules. One is generative and causes increase by division and rejuvenation, and the molecules that regulate these processes are termed generatules. The other is nutritive, regulated by ergatules which are working molecules. The former may change into various kinds of the latter, and, as they contain chemical radicals for all ergatules, they determine their nature, and hence, indirectly, all the properties of the body. The generatules are far more alike, but may change, especially where they are most numerous in the nucleus. The ergatules are very diverse and abound in the body of the cell. Their different kinds are distributed by division to the cells of different tissues according to the functions of the latter. They are derived from primary ergatules which are less differentiated. Outer influence can act on the generative substance in the chromosomes, not directly but only through the ergatules, which are nearest the germ plasm, and receive and transmit the net resultant of them chemically to the germ. This influence of the environment is effected by splitting off still smaller molecules called ergatines (after the analogy of antitoxines) and these directly effect changes in the composition and architecture of the generatules, which cause variation. Primordial vital substance had only generatules and, hence, could only multiply without many phasic processes of growth, and its biomolecules were quite likely far smaller and simpler than those of higher forms of life. As the body develops from one cell, so generatules and later ergatules are always evolving and organizing themselves into cells.

Semon[2] develops in a very able book a psychic primordium. If a stimulus, when it has ceased to work, leaves an after effect upon an organism that persists, this change is called an engramm, and the sum of all inherited and acquired effects in a special line is called a mneme. The simplest bit of living mat-

[1] Berthold Hatschek: Hypothese der Organischen Vererbung. Engelmann, Leipzig, 1905.

[2] Richard W. Semon: Die Mneme als erhaltendes Princip im Wechsel des organishen Geschehens. Engelmann, Leipzig, 1904.

ter, and also all tissues are receptive, but the nervous system is specialized for the engraphy of mnemes. These processes, like many others, are best studied where the division of labor among cells is most developed, because here functions are simpler and clearer than in rudimentary forms of life. Thus the higher is the key to the lower. When two sequent stimuli are often repeated the first may come to produce the effects of both in the organism, and this is eckphoria, which later and introspectively we call association. Both engraphic and eckphoric effects may be caused by fainter and fainter stimuli and then some may act automatically as habituation in response develops. There are also latent effects observable only after the summation of many stimuli. Chronogenic eckphoria underlies and explains seasonal changes in plants and animals, such as ovulation, migration, etc,, while phasogenic eckphoria is the influence of a certain stage of development, like, for instance, puberty upon other changes set up by it, each stage of growth, *e. g.*, stimulating the next by means of the engramms of the race or species in each individual. Thus engramms persist through generations which are phasic repetitions of them and their combinations. The instinctive but adapted actions of young and lower animals are due to masses of inherited engramms. Weismann[1] criticises this theory, which he terms "brilliant and *geistreich*," because, unlike his determinants, engramms do not originate in the germ plasm only but in the soma as well. The engramms of the germ plasm are toned down images of the experiences of the rest of the body, transmitted to it from its various parts, and which become effective at each stage. Engramms are ever pressing germward from every organ. Semon does not know how, but collects many facts which he thinks show that they do so, such as the persistence of a daily rhythm in plants developed from the seed in constant darkness, or in plants transferred from north to south, or *vice versa*. Thus Semon's view is almost the polar opposite of Weismann, since, in fact, everything for the former is the result of the acquired experience of the individual or the race registered upon germ plasm. Upon this view memory is made the key to heredity and also to the states and processes of the "upper consciousness," which must also be explained, although in doing so we must sedulously avoid all terms of introspective origin.

While the above are the more important theories of their type, it may be interesting to résumé from my notes a few similar hypotheses which are either older or less elaborate.

[1] A. Weisman: Richards Semons Mneme und die Vererbung erworbene Eigenschaften. Archiv für Rassen und Gesellschafts-Biologie. Jan. and Feb., 1906. 3 Jahrgung S. 1-27.

Among these we must place Bechamp's[1] ingenious microzymas which are of very different sizes, normally spherical, although they may temporarily take on different forms when closely apposed to each other. Their chemical composition is different and they may undergo specific changes in new conditions. They only really live, and death is the disaggregation of their temporary associations. Yet they cannot die but live on ready for new combinations, and in this form they pervade all earth and sea. God created them. Their chief power is to secrete soluble ferments. This action is essentially dissolutive and they evolve products of decomposition. They strongly tend to associate into organisms of ever higher order. Their lowest amorphous aggregation is glaivine in mother of vinegar, wine ferments, and next higher, in bacteria where they fuse into colonies of chaplet and other form, and third they make cells. Darwin's gemmules move about and penetrate all parts of the body until they reach just the right cells. Each represents the cell in which it arose and they give to the cells they inhabit the power to reproduce themselves. There is an incessant fecundation of cells by gemmules, but only germ cells receive gemmules of all the cells (pangenesis). Outer impressions tend thus to be reproduced in the sex cells, so that these change with the conditions of life. As growth progresses each new cell at birth receives more of the gemmules destined for it. It is, however, proven that gemmules are not transported in the nerves or in the blood, so that the weak point of this theory is how they so freely permeate the body. Hertwig's[2] idioblasts are bits of nuclear hereditary substance that grow and multiply primarily without otherwise changing. They represent all the elementary properties of the cell, each receiving not only just what it needs individually, but, as it were, sparks of all sorts of other ideoblasts that are characteristic of the individual. Some are active in some cells and some in others, according to the environment. In reproduction they fuse and do not juxtapose so that offspring are intermediate between the parents. In each there are dormant all the traits of the race, any of which another environment might have developed. Thus evolution is epigenesis or due to impressions made by different experiences upon originally identical material. Hence his view is opposed to that of preformationists like Weismann. Wies-

[1] A. J. A. Bechamp: Les Microzymas dans leurs rapports avec l'heterogenie, l'histogenie, la physiologie et la pathologie. Lille, Paris, 1883.

[2] Wilhelm August Oskar Hertwig: The Biological Problem of To-Day: Preformation or Epigenesis? translated by P. C. Mitchell. W. Heinemann's Scientific Handbooks, London, 1896.

ner's[1] plasomes are also the smallest vital particles that can reproduce themselves, for that is the cardinal biological function, although they must also assimilate and grow. We cannot tell whether they are eternal or spontaneously generated or whether each has a psychic element. Constituting everything that is alive, they differ immensely from all that is not so. They tend to mass and thus increase in size by intussusception. Most die, but not the gemmiplasm which persists and is little influenced by the environment. This is chiefly a growth theory and little more than that. Haacke[2] thinks that under polar forces living matter tends to crystallize into gemma and these plasm crystals into gemmairs. He gives geometric cuts of groupings, and between these forms and those shown in the cell during its development traces resemblances. The arborizations and other forms these elements take are explicable by the law of equilibrium which is to them self-preservation. Although there is an impulse toward perfection, biology is hardly yet an independent science. Verworn's[3] biogens are living molecules formed, after the analogy of chemical compounds, from lifeless matter. They are permanent, although their composition is ever changing, and are thus comparable with the flame of a candle. They have great power to leaven all that they reach with life by catalysis. These enzymes, as with Ostwald, control digestion, reproduction, and are a kind of energids that accelerate oxidization, Many of them are coordinated and highly specialized in each cell. Maggi[4] starts with amorphous glia which corresponds with Haeckel's unindividualized autoplasm. It is made out of organic matter and from it are first differentiated plastids which are free like bacteria, but which later became associated in monera and yet later combine into vital granules. E. Giglio-Tos's bionomes[5] are vital units which combine into biomads and then into cells. Jäger long ago thought that all dermal and glandular emanations either were, or were very closely connected, with ultimate germinal particles, and spent great ingenuity through two large volumes in working out the relations between sex and smell.

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[1] Julius Wiesner: Die Elementarstructur und das Wachsthum der lebenden Substanz. A. Hölder, Wien, 1891.

[2] Wilhelm Haacke: Gestaltung und Vererbung. Chr. Herms, Leipzig, 1893. See Die Träger der Vererbung. Biol. Centralbl. Juli, 1893. Bd. 13, pp. 525-542.

[3] Max Verworn: Die Bewegung der lebendigen Substanz. G. Fischer, Jena, 1892.

[4] Leopold Maggi: Protistologia, No. xxxiv of the Manuali Hoepli-Serie scientifica, Milano, 1882.

[5] Ermanno Giglio-Tos: Les problemes de la vie. Cagliari, chez l'auteur, 1900-1905.

From Plato's *nous* to von Helmont's *archæus*, animistic theories ruled all attempts to explain life, and later the great school of Montpellier gave wide currency and momentum to theories of vital force. The spermatists and ovists then long divided the allegiance of thinkers. After the microscope opened a new world and chemistry had proven the existence of bodies far beyond its ken, the scientific imagination took flight in micromeristic constructions, some of which, as we have seen, have been brilliant and stimulating in the highest degree. These invisible wonder-workers, if they exist, are the artists of life, older than all fauna or flora, and their remoteness in time is hardly less impressive than their special minuteness. Whether they are mind stuff or real things, they show that man's psychic powers can vie with nature herself in producing marvellously intricate structures, and they make a certain æsthetic as well as logical appeal to the mind. Some of them are wrought out with amazing cleverness and with immense labor and have great explanatory value, while others are cheaper products, but all shed new light upon the categorizing instinct of the human reason, and contribute something to realize the richness, worth and immemorial antiquity of life and of the soul, henceforth one with and inseparable from it.

All the above views transport the most basal and interesting problems of both life and mind to the inconceivably remote in time, small in size and to matter, whether conceived materially or dynamically, till they seem tantalizingly transcendent and the field of ordinary experience appears cheap and rude, a kind of reliquary of nature where and when she was in her creative prime. What used to be called brute matter, for which thinkers have been almost misophobiac, now seems nearer the great first cause, full of far more possibilities and potencies than have been realized, and so much above and so far more complex than mind that science cannot even understand it. Perhaps now, with the marvellous new views of the ultimate constitution of matter, we exalt rather than degrade the soul and life just in proportion as we explain them by physical and chemical forces. Reason is not adequate to grasp these, and our cleverest thought and imagination only formulates our ignorance and is less, not more, than fact. Matter seems more than life, life more than mind, and germ plasm far beyond the power of the brain or soul to comprehend it. Mechanism is most and life the least knowable.[1] We cannot know self in any fundamental sense till we know protoplasm or the amœba, and evolution is only the best case of devolution. In this new orientation and trans-

[1] Konrad Guenther: Darwinism and the Problems of Life, tr. by J. McCabe. Owen, London, 1906.

valuation, it is the undevout materialist and germ-plasmist who is mad, and even dissolution and death lose some terror and acquire some charm, for we are in the end at least resolved soul and body into some diffusive powers, which are the "mothers of life." Which way really lies up and down, progress and retrogression in the vast continuum we call the universe, but which may have no boundaries or breaks in either time or space? Again, the inveterate fallacy of students of the soul, with the exception of a few bolder spirits mentioned above to explain everything by the nearest cause, is in striking contrast with the tendency of biologists to utilize the new horizons of distance and size, although, save the few who accept Weismann's[1] biophores and determinants, almost none of these have followers. As we know it chiefly in cells, protoplasm is no longer primal, homogeneous or structureless, but is pervaded, if not constituted, by a slowly accumulated mass of heredity, with thousands of latent traits, for to live is to habituate. Their momentum still dominates and all that has followed since is relatively easy and brief. Every bit of protoplasm has a long history, or is, in fact, an historic structure, and trophic and reproductive primordia still impel to variations that are independent of present utilities or of recent environment. From these phyletic structures assimilation and dissimilation, growth and reproduction, the most fundamental traits of life, the bases of hunger and love, arose before the oldest fossils, so that their pedigree is lost.

Geike[2] puts the formation of the solid crust of the earth at little less than 100 million years, shortly before the seas were developed and the plankton on its surface, where Brooks[3] thinks life evolved, was formed. Joly[4] thought 55 million and Dubois 36 million years the probable time since river beds began, and Lord Kelvin,[5] in 1862, estimated from the rate at which the earth's crust cooled that nearer 20 than 40 million years must have elapsed, and many estimates of increasing temperature downward must have approximated this calculation. George Darwin[6] concludes that the moon broke away from the earth some 56 million years ago. How can we reconcile the fact that the first life was doubtless aquatic with the chiefly igneous nature of the oldest rocks. I can only suggest that perhaps the convulsions caused by the breaking away of

[1] August Weismann: Essays upon Heredity. Clarendon Press, Oxford, 1891-92.
[2] *Opus cit.*, p. 153.
[3] William Keith Brooks: The Foundations of Zoölogy. Macmillan, New York, 1899.
[4] *Opus cit.*, p. 153.
[5] *Opus cit.*, p. 153.
[6] *Opus cit.*, p. 153.

the moon from the volcano-girt Pacific, which Pickering thinks occasioned the Americas to split from Europe and Asia, the general contours of which are so conformable, made the oldest rocks so metamorphic that in the heat and the mixture of their semi-moulten magna all the earliest records were obliterated. This would explain the fact that, when the crust petrified and the earlier clear palæontological traces were laid down, we have so early so nearly all of the great branches of the animal kingdom, although the highest are represented only by their lowest and most generalized forms, especially in the case of vertebrates. Despite this cataclysm, some of nearly all the then existing species must have survived in some locality as in a veritable Noah's ark. If, as de Vries[1] thinks, species arose by mutations, and these in early times were greater, in more directions and with shorter intervals between their periods, the geologic time available might more easily suffice, while a longer period would be required if variations were very gradual.

It is a common view that the lowest organisms are most persistent and least variable and that many of them have not changed since the first appearance of life, but have continued to breed true to their primitive type. Weismann, Poulton and Darwin held this view. The latter was "sorely troubled" at Lord Kelvin's limitation of geologic time and would have preferred Fry's estimate[2] of 2,700 million years since life began, the assumption being that evolution started very slowly and was accelerated as it advanced to higher types. But, instead of being most rigid, should we not rather regard these earlier forms, when heredity was, as it were, young and had not acquired its momentum, as most plastic and least stable, as more are now coming to think. This view Bastian[3] has lately wrought out with great boldness and ingenuity. The apparent persistence of simplest forms he thinks due to incessant *de novo* production from non-living matter, just as crystals arise wherever conditions are fulfilled. Instead of being Silurian, countless ephemeromorphs are always arising, a peculiarity of their molecules being to take on life forms. These are ever arising in great profusion by archibiosis. The lower of them are heterogenic and pass into each other by transformation where *per saltum* variation may even cause a rotifer's egg to develop into a ciliated infusoria. To this class of cases much of Bastian's book is devoted. When we conceive an atom as the core of a

[1] Hugo de Vries: Die Mutationstheorie Veit. Leipzig, 1901.
[2] Sir Edward Fry: The Age of the Inhabited World and the Pace of Organic Change. Monthly Review. December, 1902, Vol. IX, pp. 42-53, and January, 1903, Vol. X, pp. 68-83.
[3] H. C. Bastian: The Nature and Origin of Living Matter. T. F. Unwin, London, 1905.

group of positive electrons, like a sun, around which negative particles revolve in orbits like our planets, and an atom of hydrogen as different from one of iron, *e. g.*, only in having more planets or different orbits, so that new elements like thorium, uranium and perhaps helium are undergoing spontaneous generation, why, asked Bastian, should we deny mutability, plasticity and spontaneity to the vastly more complex foramenifera? Rapid and immediate response to changed environment, with the influence of the past less and that of the present greater, causes in low forms of life alterations that seem fitful and lawless, but these preform the discontinuity of variation and the mutation which Bateson and de Vries find in higher forms. Both are leaps across perhaps not yet well settled taxonomic demarcations. This, too, would account for the universal distribution of simple forms. The similarity of old and new types is due to like conditions. Bacteria may occasionally arise spontaneously, so that not every germ disease is due to contagion, and so may some torulæ and molds. Links of relationship that unite organisms are therefore not all hereditary, but are partly due to uniformity of laws acting under uniform conditions, or to constant matrices which are ever fertile. Of these ever upsurging new forms the highest animals arose from oldest, man with his brain and reason being very likely as aberrant and transitory as the monstrous reptiles of the Trias and Jura, the beginnings of the far higher forms that will supersede him being perhaps in the act of arising from matter to-day. Finally, Bastian agrees with Newcomb that among all the countless stars there must be "thousands and perhaps millions" on which the conditions are so like those on the earth that life surely exists, though doubtless different, in some worlds higher, in others lower, than here, but in all different.

If the way up to life, being once found, is still kept open and is still traversed as much and even as slowly as at first, this, of course, does not affect the view that its original construction was long and hard. There may be many ways and those now used may short-circuit the old ones, or the best may be selected and all the old ones discarded. New plasm may be formed by rapid recapitulation of the mode of origin of the first. Protoplasm must be of innumerable kinds and compositions and the new, although undistinguishable from the old, may be essentially different and without all its promise and potency. The first plasm may have exhausted itself in giving birth to the higher species, and the persistence of low ones may be due to progressive incapacity of nature to equal her first effort, so that when the higher forms have vanished only attenuated, abortive and ever simpler ones will be left, life as a whole dying from the top downward, as old men produce ever less effective sperm.

The power to breed true may decay and only late and novel forms may transmute, indeterminate heterogenesis marking decay at the root; or, conversely, life in the far future may be more vital and both the plasm and species of to-day be the products of nature's prentice hand, so that the phylogeny of the future will recapitulate that of the past with a much increased rapidity and economy as the individual recapitulates the history of the race, perhaps even higher species transmuting into others. Dreameries like these do not begin to exhaust the speculative possibilities in the field where so little is known, so much is possible and the hunger for new light is so great. More seriously we may ask how, if life is ever arising anew, we can explain the law of both palæobotany and palæozoology that if a species dies out it never reappears in a later geologic age; also the fact of life-zones and of the disparity between homotaxy and contemporiety, now well accounted for, would be harder to explain[1] than if all existing protoplasm is derived from previous protoplasm and is now so complex that its present form must be the result of a very long period of development since it first became protoplasm.[2]

Plants, says Fechner,[3] might well ask whether restless human bipeds were of any use in the world save to prepare carbonic acid for their leaves and when they die to furnish with their rotting bodies nitrogen for their roots. Plants eat, spread and fertilize their seeds, and insects are only love messengers to bring pollen to the female blossom. The plants could exterminate the humans by sending forth a bacterial army. Fechner was led to postulate souls in plants as one result of his conviction that the psychic elements of individual life could be preserved after death in a high unity. Indeed, under all physical processes he assumed a psychic rudiment which must cross more than one threshold to become conscious, so that there must be a psychic corresponding to the physical continuity. Fischer[4] thought the word "*eidos*," idea, meaning form, suggested that there is something psychic in every organization of a type or species which is comparable to a school of art or philosophy. Motion had no beginning and is the life of the world, and rocks and earth are a cadaveric rigidity after the secretion of which protoplasm is left over. Life is persistent, not gener-

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[1] H. Nichelson and R. Leydekker: Manual of Palæontology. 3rd ed. Blackwood, Edinburgh, 1899.

[2] O. Hertwig: The Cell. Tr. by M. Campbell. Macmillan Co., New York, 1895.

[3] G. T. Fechner: Nana oder über das Seelelenben der Pflanzen., 2 te Aufl. Mit eine Einleitung von Lasswitz. L. Voss, Hamburg, 1899.

[4] Fischer: Ueber das Princip der Organization in der Pflanzenseele, 1893.

ated, but sprang from the absolute basis of all being where it existed in potentia so that there never was a time when all the world was dead, and plant souls are the summation of molecular souls. Némec[1] thought he had demonstrated structures and functions in plants not unlike those which Apathy found in the higher metazoa. He concludes that many vascular plants possess special structures for conducting stimuli in the cytoplasm of their cells, so that the similarity to like processes in animals is far closer than had hitherto been supposed. After demonstrating structures that seemed homodynamic, his conclusions were confirmed by studying experimentally the propagation of stimulus, and this indicated fibres set apart for this purpose, for their bundles could be followed directly to the seat of the bending movements, and both fibres and the function of quickly responding thus vanished again. He finds just under the dermal surface of the root cells sensitive to geotropism, and these cells are characterized by the presence of grains of starch which serve to orient the root to gravity. Their specific weight is greater than that of the protoplasm of the cell, so that they press against different parts of the surface in different positions of the root. The stimuli is transmitted with undiminished intensity, and in some plants these movable starch grains are developed to a special organ comparable with the static organs, which in metazoa are provided with statoliths. This author agrees with Czapek that there is no difference in principle between many reflexes of plants and those of animals, and that not all movements of plants can be regarded as antetypes of metazoan reflexes.

Before and especially since Darwin showed the adaptive movements of plants, their many modified forms of circumnutation in epinasty, hyponasty, nictetropism and sleep, their sensitive responses to light, heat, gravity, moisture, contact, etc., compared their movements to those of animals, the tip of branch and root to a brain, studied their cunning devices for catching and digesting insects, their reactions to their visits in cross-fertilization, and their plasticity under domestication, botanists have applied many experimental methods and measurements which show that if plants do not have a soul, we need only to magnify their movements in space and condense or accelerate them in time to see that their motor reactions give them an exquisite rapport with their environment, which is as good as, if not better than, psychic. The brilliant and epoch-making experiment of Bose[2] goes much further and

[1] Dr. B. Némec: Die reizleitenden Strukturen bei den Pflanzen. Biol. Centralblatt, June 1, 1900. Bd. 20, pp. 369-373.

[2] Jagadis Chunder Bose: Plant Response as a Means of Physiological Investigation. Longmans, Green & Co., London, 1906.

shows that there is no important reaction of the most highly organized animal tissue that is not also found in plants. Their motility under electrical and other stimuli is not confined to young plants, buds or leaves, but they are very sensitive to fatigue, which has its curve of excess and recovery. Every stimulus has its latent period, its threshold, its optimum conditions, and may be summated into actual tetanus. Plants are sensitive to many drugs. Alcohol exalts or depresses their functions as it does those of higher animals. Some have sense organs that can be located. Many of them pulsate with waves of turgidity and with regular, though usually rather slow, systole and diastole and thus they circulate sap. Plants may be killed, although not without a marked preliminary death spasm with subsequent *rigor mortis* followed by relaxation. They die more easily if fatigued. The rate of transmission of a wave of stimulus is measured. Sap rises from the root to the highest tree top not chiefly by capillarity, but by active suction, also measurable by special apparatus. Growth is rhythmical, consisting of a wave up and another back, the real growth being the excess of the first over the last. Plants can be habituated after a refractory period. Their sleep is caused not by darkness, but by real exhaustion. Their tropisms to sun, earth, currents, their power to twist and twine is affected by age, previous activity and many other circumstances. Excitation is transmitted from one part of the plant to another by lines of protoplasmic reaction, along which the stronger the stimulus the greater the velocity. In both plants and animals the anode blocks transmission and most motile response is all or none, as with the animal heart, while acids and alkalies have an action quite akin to that of the heart. In all these reactions there are characteristic differences between species, old and young plants, and sometimes between individuals of the same kind and age. Even the author's experiments with such common plants as celery and lettuce show a behavior in them that can hardly be entirely excluded from psychology. Thus the unity between plants and animals is fundamental and detailed. Everything in the latter is at least begun in the former so that experimental psychology must henceforth have a botanical section. All is vital and yet all becomes mechanical in proportion as it is known. The general order of phyletic development in the plants was first algæ or perhaps fucoids, then acrogens, conifers, cycads, palms, with phænogams and their fertilizing insects late, and it would be an interesting next step to know how the physiology of early and late plants compares. It is only because our studies of the mind have been so chiefly human, adult and analytic of the consciousness of the present moment that they are too provincial to see the ocean of soul of

which our mind, which we think is so supreme, is but one outcrop, or that we find it so hard to realize that upon any large and sound definition of soul life, more of it existed and perished before man appeared than all that has arisen since. Wherever there is vitality there is *psyche*, and science not only shows nothing to contradict this deep instinctive belief that crops out in animism in all primitive religions, in poets, ancient philosophers, is imbedded in the structure of language, myth and popular opinion, but plants show not only what Aristotle called the nutritive and reproductive soul, but add to these subtle responses to all the influences of their *milieu*. The psychic life they show is the oldest and most dominant part of the soul in our narrower sense of that term and here, therefore, we must find the apperception organs for all the rest of our knowledge of the latter. Man's secondary consciousness is only one of the countless allotropic forms of soul, although even it may by an act of faith be postulated as but one of the many forms of energy sometime to be explained under the law of cause, and all perhaps quantitatively. Just so far as psychology becomes a natural genetic science it will trace all higher powers back to those we have in common with plants and the simplest animal forms, and *vice versa*, derive all the former from the latter.

The earliest plant life was very abundant, but of few kinds, and those so perishable that palæontological traces are rare. Indeed, vegetation has been chiefly terrestrial and its marine forms low in type. The simplest of these are very plastic and can subsist in water, hot or cold, saturated with salt, lime, sulphur, silica, and about everything soluble. Some can endure freezing and great desiccation. Bacteria, though derived and not original forms, must have existed to account for decomposition. The peat mosses and then the ferns and equisetæ, lycopods of the coal age, and later yet the seed plants or spermatophytes on which animal life is most dependent for food, was the order. As opposed to the common view, some now hold that life originated on land and in fresh waters and migrated to the sea. Despite his control in agriculture, plants have done a vastly greater geologic work than man and are far more plastic to diverse conditions than are animals. The latter is doubtless especially true of the first forms of which so little is known. While it is unsafe to infer from the responses of the very high and sensitive plants of tropical India, with which Bose found his most striking results, to primitive plants, the botanical realm probably shows no exception to the general rule, true also for zoölogy, that the responses of lower forms of life are relatively more in structure and those of higher forms in function.

Every detail in both the life and form of every plant is due to stimuli from its morphological environment. Heredity is the innateness in the individual that was acquired by the species. Probably all these structural and functional adjustments to conditions were originally compulsory. Inflorescence, *e.g.*, is the condensed result of a slow modification of leaves into petals, each stage of which had some external cause. Reproduction and all its intricate stages, types and attendant phenomena was impressed item by item upon substance endowed with two traits alike marvellous, viz., plasticity to receive and persistency to retain. Growth, size, shape, texture, position of leaves and buds, color and every other item which distinguishes one species from another, are one and all the accumulation of what were primarily individual reactions to light, temperature, moisture, chemicals in air and soil, contact, gravity, etc. All differences in plant structure are massed effects or records which science must read of the way and degree these forces have acted upon them or their ancestral forms; hence, to know plants completely is to know their history. In other planets where these incident forces were differently composed or proportioned, plant nature would be changed exactly as much but no more than they. Moreover, an individual life is not the unity it seems, but an aggregate of units, or, as Bateson[1] calls them, allelomorphs, which are often of very diverse origin, independent one of the other, but which in reproduction may be combined in every possible way. While in old established forms acquired qualities modify heredity only very slowly, so that Weismann is essentially right that the net results of individual life upon germ plasm are minimal or naught, the past determining everything, only Lamarckianism in its most extreme form can explain the evolution of races, species and their every diversity, great and small. Hence, preformation theories lack chronological perspective in the same way, though in less degree, than does creationism itself. If every change from fertilization to maturity is dependent upon a series of stimuli, instead of being spontaneous as extreme experimental embryologists think, then all the

[1] "Each such character, which is capable of being dissociated from or replaced by its contrary, must henceforth be conceived of as a distinct *unit-character*, and as we know that the several unit-characters are of such a nature that any one of them is capable of independently displacing or being displaced by one or more alternative characters taken singly. We may recognize this fact by naming such unit characters allelomorphs. So far we know very little of any allelomorphs existing otherwise than as *pairs* of contraries, but this probably merely due to experimental limitations and the rudimentary state of our knowledge." W. Bateson: Mendel's Principles of Heredity. University Press, Cambridge, 1902. p. 27.

more must this have been true of the development of species for which stimuli must have been relatively more and response slower and more uncertain. From this new view point psychology must henceforth study all structural and functional adjustments as the key to all perspective and reflective adaptations. The moving equilibrium that pervades animate nature differs from human intelligence in nothing fundamental save that in the former new adjustments to new influences in the environment are slower in time. What we call consciousness is derivable from the suddenness or the biometamorphosis with which new balances are attempted and attained and to the more complex and manifold changes that this involves in higher organisms which are susceptible to shock in a different way. Thus the genetic psychologist may regard man, as von Baer has suggested, as at bottom a vegetative being (most of his organs concerned with metabolism, circulation and reproduction arising from the entoderm on which is superposed an animal or psychomotor being, the senses and brain and, to at least some extent, the muscles, arising from the entoderm).[1] In some sense we may speak of latent mind in vegetation or of life as nascent mind and mind as patent life and in so doing lay a new fillet of wool on the creations of Schelling, Fechner and even Bruno. and admit that speculative sagacity and even oriental pantheism, like that which animates Bose, are not to be regarded as *a priori* anti-scientific.

Of the early pre-Silurian animal ancestors of man from unicellular protists up to the oldest skulless, jawless, limbless monorhine vertebrates, Haeckel makes fifteen progonotaxic stages, of all of which palæontology shows no trace, although each has living relatives. He admits that other pedigrees might be made, although for some he expects further knowledge to verify his hypothetical forms. In the oldest Archæan age, of which, if we accept the planetesimal theory, there can be no assignable lower limit, although there are no fossils, there is evidence that life was abundant, while little is known of its character and it is separated from the next age by a great unconformity, as is the next or proterozoic age from the palæzoic, where abundant fossils are first found. It is believed that the earliest were algæ, rhizopods, infusoria, plastoid, with multicellular hollow vesicles. Later came gastrula forms with two germ layers; then primitive worms of an ascending order, ending with those that showed anticipations of a notchord, and then the low skulless vertebrate. During this period the intestinal cavity, a respiratory tract, which was first in the gut

[1] Ernst Haeckel: Evolution of Man. 5th edition trans. by J. McCabe. Putnam, N. York, 1905. 2 vols.

itself, renal functions and the flexible axis between the intestine and a ganglionic chain which was the *Anlage* of the spinal chord, were developed. Of course Haeckel abhors the inverted evolution of Dohrn,[1] who has himself since repudiated it, that the amphioxus, thought to be the parent of all vertebrates, was a degenerate cyclostome, that even ascidians and tunicates are decadent fishes and that all animals are the progressively fallen offspring of man who was at first sole and supreme. He thinks the tunicates, although degenerate, are the nearest blood relatives of the vertebrates, although only from a common root and not in their line of descent. There is no doubt that touch, smell, complex movements, rudimentary vision and taste were developed in the primordial sea before the age of the oldest fossils. Life histories, habits, reproduction, recapitulation by the individual of the traits of its race were established in a fundamental though very simple way, and movements that were adapted to needs and adjusted to stimuli were developed and concatenated.

It is even harder and more conjectural to select from the psychoses of living lower forms those traits that were near the beginning of the line of descent of man's mind than it is to trace this stage of the pedigree of his body. Probably the food quest is the first field of development of an individual *psyche* and this is often quite intricate in the protozoa. An amœba can squeeze an acineta to obtain the young from the ovarian aperture, which it seizes as soon as it emerges and ultimately digests. Paramecia gather about a bit of bacterial zoöglœa, show antipathy to alkali, troop about a drop of dilute acid like a crowd about a popular orator, maintaining an optimum distance where the intensity suits them, although they often seem to mistake bits of filter paper for food. Actinophrys watches for the spore cells of pythium as the ciliated monadic germs emerge, captures and devours them all. Actinaria wave their tenticles inward in the presence of soluble food so that Jordan[2] thinks they taste, Romanes[3] that they smell, Nägeli[4] that they are only stimulated muscular action, Loeb[5] that the ciliary surfaces are also, if not chiefly involved. Parker[6] shows that

[1] Anton Dohrn: Das allgemeinste Gesetz der Natur in alles Entwicklung, 1864.

[2] David S. Jordan and Vernon L. Kellogg: Animal Life. D. Appleton & Co., New York, 1900.

[3] George J. Romanes: Jelly-fish, Star-fish and Sea-urchins (International Science Series). D. Appleton & Co., New York, 1885.

[4] Carl von Nägeli: Mechanisch-physiologische Theorie der Abstammungslehre. R. Oldenburg, München and Leipzig, 1884.

[5] Jaques Loeb: Vorlesungen über die Dynamik der Lebenserscheinungen. J. A. Barth, Leipzig, 1906.

[6] T. Jeffrey Parker: Lessons in Elementary Biology. Macmillan, London, 1901.

in some kindred forms the waving of the lip-cilia caused peristaltic gullet contractions of the sphincters of the oral disk. Hodge[1] thought that vorticellæ, after filling up with yeast torulæ and violently disgorging them when found unfit, learned to avoid them. Some phenomena ascribable to thigmo- and chemo-tropism and even to ions are really due to the food instinct. This primitive hunger cannot, yet, at least, be explained as endosmosis or as chemical affinity between the animal body and its food as Dantec suggests. If the psychologist can say of any element of the soul that it was first, it is something which has developed, through we know not how many stages from some such germs as the above illustrate, into appetite, and this has ever since been one of the most potent springs of mind. It is perhaps not only the oldest but the strongest of all ontologic impulsions and involves some discrimination of touch-taste or of smell or both, as well as some differentiation between the euphoric state of satiety and the painful one of incipient starvation. The mode of getting food first differentiates animal life as mobile from plant life as sessile, so that here the genetic psychology of the individual takes its rise. It is at first about the only content of the infant soul and gives shape to nearly all forms of animal life in the struggle for survival. The first formula of will in the zoölogical world is, "I would assimilate and raise this or that to the plane of my somatic life and so grow." Perhaps when we know why one thought or feeling is preferable to another it may sometime be clear that it is at bottom because it favors cerebral or general nutrition. An animal is a creature that seeks its food by active movements of the whole or a part of its body and it is at home wherever it finds it.[2]

Another primitive motor orientation was probably caused by light. This, of course, began long before any rudimentary eye and is seen even in bodies that appear to us transparent. As early as 1878 Strassburger[3] showed that certain plant spores moved toward light at a rate determined by its intensity. Some twelve years later similar tests began to be made on lower animal forms and of such studies there is now a copious literature. Loeb[4] found that rudimentary organisms either gathered in light of a preferred intensity or parallelled the axis of their bodies to

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[1] C. F. Hodge and H. A. Aikins: The Daily Life of a Protozoan. *American Journal of Psychology*, Jan., 1895, Vol. 6, pp. 524-533.

[2] For its paidological outcrop see Sanford Bell: An Introductory Study of the Psychology of Foods. *Ped. Sem.*, Mar., 1904, Vol. 11, p. 51-90.

[3] Eduard Strassburger and others: A Text-Book of Botany. Trans. by H. C. Porter. Macmillan, 1903.

[4] Jaques Loeb: Die Orientirung der Tiere gegen Licht: Sitzungs ber. d. phys. Ges. Wurzburg, 1888. S. 1.

the incoming ray, or both. In a box, one end of which is near a window and the other darkened, some forms gathered at the light end, some at the dark and some at intermediate points, as they are positively or negatively heliotropic. Verworn[1] and Altmann found an orientation to a certain intensity of light which they thought to be more or less psychic. Miss Towle[2] studied cypridopsa in a case covered with a glass prism filled with India ink, so that one end was dark and the other light, as Yerkes[2] did entomostraca, daphnia and other crustaceans, and all were found distinctly phototactic. Such creatures tend to move to and fro and settle in the light intensity to which they are attuned or accustomed. Some of them change from positive to negative response and they alternate between the two. Choice is often affected, as is the retina, by the degree of light from which the creature has just come. As to color, Burt and Lubbock[2] found daphnia preferred yellow and green, Engelmann[2] that navicula seemed to have preference in the following order: red, yellow, green, blue, violet, although intensity may be a more important factor than chromopathy. For some low orders of life it seems that the limit of susceptibilty at both ends of the spectrum is nearly that of man. Graber[2] found marked response to color in the simplest forms of life. Some of these earlier experiments did not sufficiently eliminate the heat differences of the different spectral colors, nor consider the movements and attitudes of the features to be tested with reference to the direction from which the rays came, nor did they determine the shock causable by sudden changes of light. But it is certain that eyeless, nerveless beings thought to be more or less like the very earliest human ancestors are keenly alive to light in ways not yet explicable photochemically save by theorists, as if some optical function were early diffused throughout the body. Light preferences may have been affected by the sea level to which the creature was accustomed and have been to some extent both cause and effect of its habitat. Perhaps, too, in palæozoic times the atmosphere made the quality of daylight, and possibly even night, very different from what it now is, but, although some amœboid forms of life seem to show little diurnal rhythm, for most grades of existence day augments and darkness depresses activity. It is not known that even this ancient change ever produced true ephemeridæ whose spontaneous life was limited to one day. Thus the chief time markers, day and night, early began to cadence life to their rhythm which has since developed into sleep and waking. Life entirely in the dark is probably derived and not original.

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[1] Max Verworn: Psycho-physiologische Protisten-Studien. G. Fischer, Jena, 1889.

[2] For complete reference see p. 212.

Hence a light appetency or hunger arose with optima more or less definite and specialized, and the phototropic eye of the new-born babe, like everything else, has its long ancient phyletic history of which we as yet know so little. Even light was probably at first ancillary to metabolism.[1]

Primitive organisms respond, some of them, very exactly to thermal stimuli, seeking the optimum of temperature most favorable to their life processes. Placed in troughs, one end of which contains ice, with a lamp under the other, they orient and migrate, settling where conditions are best. They can be acclimated in temperatures more or less different from all those to which they are wonted. They have their upper and lower limits above and below which vital processes are suspended. Contrast effects are produced by which those coming from colder environments seek higher and those from warmer seek lower temperatures; also habit effects by which they settle at points in the scale most nearly like those they have just left. Both these have their respective human analogues when men warm themselves after cold and seek coolness after heat and also find difficulties in adjusting to tropical or polar climates. Some are acclimated and others can adjust to far greater thermal extremes than man and some to less. Sympathy began with warm-blooded animals, and to keep their young warm is one of the primordial factors in parental care and love. The social instinct, too, as Sutherland has shown, is partly due to huddling for mutual warmth.[2]

Contact is the only stimulus that may be older than hunger, or the three states of matter, solid, fluid and aerial or gaseous, life doubtless began in the second, and it is conceivable that its haptic experiences may have been often limited to changes of pressure due to diverse rates of movement in water. Where thigmotic and stereotactic impressions occurred, they were with films, surface tensions, food, each other, or with enemies, and only rarely and later with shore, bottom, rocks, etc. The almost sole movement of a primitive group of simple forms, of which the paramecium is a type, is a forward push, then on meeting an obstacle a backward thrust, a turn to right or left at an angle dependent on the stimulus, with slight rotation of the body, and then forward again in a new direction, and the reaction is the same to chemical changes, surface, electric and

[1] For the outcrop of this psychosis in infancy, mythology, etc., see the author's study with Dr. T. L. Smith: Reactions to Light and Darkness. *Am. Jour. of Psy.*, Jan., 1903, Vol. 14, pp. 21-83.

[2] For the recrudescence of this ancient pyrotactism in children and its rôle in human life, see the author's study with C. E. Browne: Children's Ideas of Fire, Heat, Frost, and Cold. *Ped. Sem.*, Mar., 1903, Vol. 10, pp. 27-85.

other currents. For Spencer the primitive experience is a bunt and the first psychic progress consists, perhaps, of an incipient sense of light and dark, or in anticipatory touch, which is the mother of sight and all the other senses. Bottom creepers love crevices and may develop responses to points, curves, compression, changes of permeability, cohesion, thickening, etc. (aero-campo-tropism, taxis, kinesis, or peran-synaph-pachym-osis, etc.). Pressure changes not only in the oral end but in different parts of the body slowly develop reactions of different intensities and kinds as they are favorable or inimical to life in various degrees, and bodies are oriented along branches or edges squeeze into holes and cracks, hug against each other, cling as parasites, habituate themselves to thick or rare fluids, appose their bodies and attach their larvæ to certain forms or textures, distinguish food by its feel, are at home in certain, while oriented toward and perhaps exploring, greater or less densities, seeking or avoiding solids, etc. Such multifarious experiences with hardness are the psychogenetic basis of the very word "objectivity," and tactility is the root of reality. Hence, the epistemology that would subjectivise things into thoughts is only a grimace or affectation because what is fundamentally motivated by, and based upon, such manifold experiences of our eozoic ancestors through ages, compared with which all the time since man appeared is but a moment, cannot be eradicated by the decadent dreameries of speculators isolated from contact with actuality in artificial conditions of life.

Reactions to chemicals must have played an immense rôle when life was young and aquatic. Sedimentary rocks were once in solution, and in volcanic action, heat melted and dissolved them, far higher tides than now eroded them. The ocean may have been much like the Sargasso sea; it may have had salt, lime, silica, chloride of magnesia, potassium, and every mineral of the earth's crust, still found at least in small quantities, dissolved in it with every vegetable and animal substance. There were oozes and muds of diverse consistency on the way to become soils, and all these conditions made the cradle in which life in its infancy was rocked. Experiments with forms of life imprisoned in a drop and under a microscope slide, by Loeb, Hardesty, Garey, Nägeli, Dantec, Schürmeyer, Bokorny[1], showed them to be variously chemo-, tropo-, taxo-, kinetic, to be acutely oriented to oxygen, alkali, carbon, narcotics, and to water (alkalo-, aero-, oxy-, narco-, and hydro-tropic and phobic). Chilomonas enter but cannot get out from a mildly acid drop which acts as a trap, perhaps because the acid reduces their strength and they dispose themselves radially toward the top and respond to the stimulus of its partial desiccation by evaporation and if the acid is strong they may whirl until they

[1] For complete reference see p. 212.

die. Oxytricha swims into a dense solution which kills it. Chemical stimuli usually act on all parts of the body at once but in different degrees in different parts. Around diffusing acid drops, there are two invisible boundaries, an inner and an outer, and crowded within the neutral ring paramecia zigzag. If they are accustomed to an alkaline fluid to which they are negative in the culture jar, and then become attracted to an acid, or else are impelled to it by the neutralization of the alkali. When transferred to still water they are neutral to most chemicals and then resume their old sensitiveness. Massart found that increased measured osmotic pressure caused fluid to pass out of the organism and a decrease into it and that this stimulus caused motion. Carbon dioxide attracts it and yet they need to excrete it. Pfeffer thinks the effects on the outside of their bodies may be beneficial and opposite to the internal effects. In general, chemical influences underly every vital functions and pervade the entire physiology of life. Such phenomena as the above are far below restoration, yet are prelusive of it, are associated with each other and with the food quest and metabolism in ways not yet understood, and suggest some of the reverberations yet persisting in man of ancient pelagic influences[1] and that one day physiology, if not psychology itself, may be expressable in chemical formulæ and show in what close rapport water life originally was with the elements.

Even electrical phenomena now and probably from the first has produced motor and perhaps psychic responses in the lowest forms of life. Verworn,[2] Roux,[3] Wallengren,[4] Kahlenberg,[5] Ludloff, Loeb,[6] Pearl,[7] and Jennings[8] have sought to show various rheo-polo-galvano and electrotonic effects, orientation with the diffusion lines of ions, the reactions generally increasing

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[1] Frederick E. Bolton: Hydropsychosis, *Am. Jour. Psych.*, Jan., 1899, Vol. 10, pp. 169-227.

[2] Max Verworn: Untersuchungen über die polare Erregung der lebendigen Substanz durch den konstanten Strom. Pflügers Archiv, 1896. Bd. 62. S. 445-448.

[3] Wilhelm Roux: Ueber die "morphologisschen Polarisation" von Eiern und Embryonen durch den electrischen Strom. Sitz. Bericht. d. k. Akad. d. Wissenchaft zu Wien. Math. u. Naturw. Classe, Bd. 101, pp. 27-228. (Ges. Abhandlg., Bd. 11, S. 540-765.

[4] Hans Wallengren: Zur Kentniss der Galvanotaxis. Zeitsch. der allgemeine Physiologie, 1903. Bd. 2. S. 341-381, 517-555.

[5] Ludloff: Untersuchungen über den Galvanotropismus. Archiv. f. d. gesamte Physiol., 1895. Bd. 59. S. 525-554.

[6] Jacques Loeb: *Op. cit.*, p. 191.

[7] Raymond Pearl: Studies on Electrotaxis. Am. Jour. Physiology, July, 1900, Vol. 4, pp. 96-121. Some Aspects of Electrotactic Reaction of Lower Organisms. Report of the Mich. Acad. of Science, 1901.

[8] Herbert S. Jennings: Contributions to the Behavior of the Lower Organisms. Columbia University Press, 1906. Macmillan Co., agents.

with their atomic weights of velocity. Some think these effects due to anions, and that they resemble those of acids, while others hold that the heavy metallic kations were prepotent. Some think the reactions of infusoria to electricity are different from those to other stimuli. Most hold this action to be cataphoric, causing the body to swell on the cathode side to which the fluids in the body are drawn. This is true of dead but perhaps in less degree of living animalcules. Paramecia are drawn backward toward the anode by a strong current while trying to swim in the opposite direction. There is a peculiar struggle between a contact stimulus and a constant electric current, the characteristic arrangement of the cilia in thigmotaxis being overcome for an instant then resuming its sway, then the electric condition dominating again. This orientation continues and the two stimuli do not give a resultant action, the cathode usually being more attracted than the anode.

Again mention should be made of geotropism and of changing specific gravity according to needs and at different stages of growth, and the depth at which the creature lives as Pratt, Davenport and Williams have shown.[1] The heavier the animal the greater the effort in swimming. It is hard for a mosquito larva to reach the surface, cypridopsis must move fast or sink, cyclops goes by jerks, sinking between each, and other forms hold themselves up by clinging with their antenæ to the surface film or to a solid object. Others regulate their gravity by imbibing or expelling water through lymph spaces or vacuolated cell regions. Thus correlation is made with density, which is associated with food habits, although a movement up and down does mean to pelagic forms what they do to land animals who live on the bottom of their sea of air, yet they are sometimes exquisitely oriented, although with no height or depth phobias like those common in man.[2]

Of course existing protozoans probably do not exactly represent the first forms of life either psychically or otherwise. They may not be proto-organisms in the sense of representing the original forms of life. Those studied above may have departed far from their starting point under altered conditions. Such reactions as are outlined in the last few paragraphs but samples the field in which all the data obtainable must be derived from which to infer primordial responses through the ages mainly before those represented by the oldest fossils. But it must have been somewhat thus that they reacted to the influences about them and to remoter teluric and cosmic forces. The rapports were so long, close and manifold, the plasticity was so great,

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[1] For complete reference see p. 212.
[2] See the author's Fears. *Am. Jour. Psychol.*, Jan., 1897, Vol. 8, pp. 147-249. Falling, p. 154.

and the retention of effects so persistent that we must seek the broad basis of the pyramid of which man is the apex here. Our minds as well as our psychological processes are derivative of these unconscious antecedents. The kinds of motor response may show great unity of formulæ and be conditioned by the shape of the body, but they are not those of a machine determined entirely by different tensions on different sides. Immediately after death the effects are different. Nor do these minute creatures react like small masses of protoplasmic substance, as Dantec thinks, but as individuals. Some respond very like muscles exercised from higher organisms, others move about as if in quest of definite objects like the white corpuscles in our blood, or like various digestive cells, and there are in vertebrates close analogues to ciliary movements. Cope,[1] strange to say, says that "The conscious cell is the primitive cell, and the unconscious cell is the independent or specialized cell," and adds, "Brain cells are the least modified of all those that constitute the soma of the metazoa, and thus they resemble most nearly the simple beings which constitute the lowest forms of the protozoa," and more lately amœboid movement, causing make and break of lines of conductivity, has been suggested between the processes of the cerebral neurons to explain associations and disassociations respectively. Watkins[2] even infers that evolution may be regarded as a compounding of minds and that man is a psychological as well as a biological colony, so that what we call psychic unity is not that of a simple thing but of a system. Verworn[3] thinks every cell, whether free and independent or aggregated in man, has something akin to mentality, so that mind is not superadded. Many hold that the physiological is a degenerate form of the mental. It is certain that the highest creatures are built up out of functions and structures of the most primitive kind and that rudiments and reverberations of the earliest forms of life are cardinal to our own, so that their study is the beginning of anthropology.

The world riddle of the lower limit of mind is far older than Des Cartes, who made the summary *coup* of declaring all animals to be automatic machines. Evolution, however, tends to trace mind further and further toward the origin of the universe and to assume continuity from the first, as it must do to work smoothly. The present recrudescence of Cartesianism had an able and representative expression in the attempt of Beer,

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[1] Cope, E. D.: The Evolution of Mind. Am. Naturalist, Oct. and Nov., 1890, Vol. 24, pp. 899-911 and 1100-1016.

[2] Watkins, G. P.: Psychical Life in Protozoa. *Am. Jour. of Psy.*, Jan., 1890, Vol. 11, pp. 166-180.

[3] Verworn, Max: Modern Physiology. Monist, Apr., 1894, Vol. 4, pp. 355-384.

Bethe and Uexküll, who proposed to objectify nomenclature.[1] For all reactions of unicelular animals and for plants they proposed the term antitypia, and for those of higher forms the term antikinesis or back movement. Receptive organs are called anelective if their states can be changed by stimuli of different quality, and elective if only a special kind of stimuli operates. Their terminology is both ingenious and elaborate. Psychology must be expressed in physiological terms. We cannot imagine what the ants feel or sense. Ziegler[2] would banish every philosophical term and rejects the idea of consciousness as "utterly worthless." Verworn in his protista studies uses the term stimulus movements (Reizbewegungen) as common to lower animals and plants, but adds to this spontaneous movements. The term *kleronomia* has been suggested for all inherited qualities which would include reflexes and most instincts as opposed to *enviontic* designating individuals acquisitions even the term psycic is called a vulgar designation for higher nervous functions. Massart[3] has given yet more detail objective nomenclature for plant and lower animal life, including the kinds of stimuli and reaction, strength, direction, intensity and results of each.

The criteria proposed for determining the lowest limit of mentality are many and diverse, and are perhaps about the best tests and keys to the philosophy of an observer. Of these, the power to choose or select one of two or more responses pleases those who emphasize free will, but is hard to apply for even inanimate objects between two opposite forces may differ in their response because of undetected and uncontrolled differences within or without, so that their action is not exactly predictable. If, and when all is known, man himself may be found to be only a complicated machine while the simplest thing that lives seems at times to show traces of freedom, and Metschinkoff thinks phagocites choose dead rather than living cells and tissues, and Wundt that they have some selective power with regard to their foods, qualities of light, etc. Those who hold vital adjustments to be mechanical may just as logically be told to draft their machine as may those who think a

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[1] Th. Beer, A. Bethe and J. von Uexküll: Vorschlage zu einer objectivireden Nomenclatur in der Physiologie des Nervensystems. Centralblatt f. Physiologie, June 10, 1899, S. Bd. 13, 137-141. Biol. Centralblatt, Aug. 1, 1899. Bd. 19, pp. 517-521.

[2] Heinrich Ernst Ziegler: Theoretisches zur Tierpsychologie und vergleichenden Neurophysiologie. Biol. Centralblatt, Jan. 1, 1900. Bd. 20, pp. 1-16.

[3] Jean Massart: Versuch einer Einteilung der nicht-nervösen Reflexe. Biologisches Centralbl., Jan. and Feb., 1902. Bd. 22, pp. 41-51 and pp. 65-75.

form of life is higher chemistry be told to synthetize it in the laboratory. The power to learn or profit by experience is another current criterion of mind, and closely connected with this are its definitions, as memory or associative memory and, with ability to rectify errors this concept usually draws the line between psychic and non-psychic far lower down even to the dawn of life for an essential trait of protoplasm is its educability by its environment. Semon's[1] mneme includes every effect of a stimulus that persists after it is removed and assumes that plasma is water to receive and steel to retain. Radical as it is, this conception is better, but highly speculative and lends itself to the very abandon of mystic dreamery, yet the scientific imagination has perhaps never found a field for such wholesome and stimulating suggestion as in the modern plasma cult, fulfilling here the highest function of theory. On the whole, it is also the most objective of tests for psychic rudiments and experimentation, and permits less ambiguity in interpreting results. As progressively exact adaptation, life itself is at least quasi-psychic, developing as if informed by an intelligence far superior to man's, with more wisdom implicit in an amœba (to know which completely would be to know all) than in the most comprehensive cosmic philosophy. A third group of tests is affectability, feeling-tone, elementary pleasure and pain. An animalcule that has attained food, its thermal and luminous optimum, and rests, must be assumed to have attained a modicum of satisfaction. But here interpretations differ widely. Is it due to the selection of a future end and of the means thereto, or is it tropism? When Norman's[2] earth worm was cut in two in the middle the anterior part crawled on with little disturbance, while the posterior half writhed as if in agony, and the same result was seen if the front half was bisected again. When the ant and even Bethe's[3] bee was severed at the abdomen, the front end where the cephalic ganglia were situated, kept on sucking honey which flowed out at the middle. It is hazardous to infer sensation from movement, and yet these are our only bases of inference and the motive of selection and the struggle for existence is assumed to be the

[1] Richard Semon: Die Mneme als erhaltendes Prinzip im Wechsel des organischen Geschehens. Engelmann, Leipzig, 1904.

[2] Norman W. W.: Dürfen wir aus den Reactionen niederer Thiere aus den Vorhandensein von Schmerzemfindungen schliessen. Archiv für die gesammte Physiologie, Apr., 1897. Bd. 67, S. 137-140.

Also, Do the Reactions of the Lower Animals against Injury Indicate Pain Sensations? Am. Jour. of Physiol., Jan., 1900, Vol. 3, pp. 271-284.

[3] Albrecht Bethe: Vergleichende Untersuchungen über die Functinen der Arthropoden. Pflüger's Archiv für Physiologie, Oct., 1897. Bd. 78, S. 509-545.

attainment of agreeable and the escape from disagreeable states. Hence with this genus of tests we can do little or nothing. To all this Wasman[1] vigorously suggests that instinct cannot be resolved into botanical tropisms. Moths often fly at right angles to the flame instead of being drawn into it as by a magnet. Caterpillars creep up twigs to their tips as if positively heliotropic, when they are hungry, but lose this power when they are sated. How, he asks, could they turn about when they have eaten the twig clean and so not die of starvation? The truth seems to him to lie somewhere between a segmental and a centrally located seat of instinct and he deems the array of objectivizing terms useless pedantries, and defends the use of analogies, with human experiences. Consciousness proper he would limit to the last stages of evolution and probably to man, and he thinks evidences of it diminish just in proportion as we recede downward from minds like our own. We must not becloud the theory of reflex action. Yerkes goes so far as to suggest that the ant with its exquisite senses, docility and social life "possesses a form of consciousness which is comparable in complexity of aspect with the human." The sanest conclusion perhaps, is that "the theory of tropism does not go far in helping us to understand the behavior of lower organisms; on the contrary, their reactions when accurately studied are, as a rule, inconsistent with its assumptions." Jennings[2] thinks that the method of trial and error is the most essential feature in their conduct. Tropism is "a fixed way of action pressed upon the organism by the direct action of external agents," each class of them having its corresponding tropisms. Stereotyped reactions do occur, but there must be some way of distinguishing error from success, for after several trials error tends to be avoided. He holds that these reactions are of a much more flexible and less machine-like character than the theory admits. The trial and error scheme "leads upward, offering at every point opportunity for development and showing even in the unicellular organisms, what must be considered the beginnings of intelligence and of many other qualities found in higher animals."

For one, I prefer to defy the current horror of anthropomorphism and to show a decent respect to continuity. Wherever the term consciousness is introduced there is confusion and no one ever yet accepted another's definition of this pro-

[1] E. Wasman: Einige Bermerkungen zur vergleichenden Psychologie und Sinnesphysiologie. Biol. Centralblatt, Mar. 15, 1900. Bd. 20, S. 342-350.

[2] Herbert S. Jennings: Contributions to the Study of the Behavior of Lower Organisms. Carnegie Institution of Washington, Washington, 1904.

tean term. Only to speculative and monodeistic minds can the question between some psychic rudiment and tropism be so put that we must cleave to the one and despise the other answer, for both are at the same time true, and both are involved in the same reactions. Each sheds light on, and perhaps is necessary to, the other as are mechanism and life, and only dogmatism, unscientific partisanship, a passion for premature conclusions and hazy faith in what some far future will show, can take sides or strive to precipitate an answer to this unusual and inscrutable cosmic riddle. To say that a miser is chrematropic; a religionist, theotropic; one who loves home, œchotropic; a sailor, pelagotropic; a drunkard, methutropic; that when we are hungry, we are sitatropic, that boys who love girls are parthenotropic- , taxic-, tonic-, kinetic-, phobic, positively, negatively, etc., would be true, but not the whole truth. Some supernal being from a distant planet large enough to need a microscope to see man, might describe us in such terms in the early stages of his knowledge of us. What is wanted is not so simple a formula, but details, variations, the origin and history of such appetency; and so summary a terminology is sure to tempt many to rest in a sense of finality, and not to recognize it as merely a set of names for our ignorance and spurs to further investigation.

Finally, sagacious as are all these experiments, and valuable as are the results, we must not forget that they are narrower in scope and less characteristic than are the data that come from patient investigation of life histories in natural habitats from birth to death. While some of the lowest of these creatures tend to respond exactly in the same way during the very act of dividing, and the young act just like the old, the conditions of experimentation are often more or less strange to them, and are often far more specific than anything in their previous experience. Just as no laboratory tests have yet been devised that satisfactorily calibrate the general ability of children and even school work and examinations fail to do so, so these lowly creatures have a free, spontaneous life outside, often with daily, seasonal and age variations. Wherever a larval state can be detected and studied, however brief it may be, its reactions are as different from those of adults as are its forms. Power to adjust suddenly to change is overestimated by the old pregenetic conception of mentality. The naturalist's method seems too simple to the modern experimenter with his new arsenal of apparatus and exact methods. Of course, some of the elementary microscopic forms could not be studied by direct observation in their home conditions and must be domesticated under a cover-glass so that the problem how the creature lives out its fully rounded life in the normal condition to which it is most

addicted is more answerable for the larger and higher forms to which we now turn.

Our next general group must, therefore, comprise roughly, the forms now thought nearest to those represented by the earliest fossils that are at once abundant, fully featured and pre- or sub-vertebrate, and which, like, *e. g.*, the trilobites of the Ordovician age (when they were represented by seventy-seven genera, declining to thirty-one in the Silurian and tapering to —in the Tertiary,—a creature which seems to have both run and swam and had tactile antennæ) are known to have passed through a series of marked developmental stages from the time when the young were old enough to fossilize and maturity. From this we may infer that the species had already undergone long stages of evolution which individual members of it were recapitulatory when they first appeared in the rocks. Psychic development in this age probably "approached somewhat nearly to that now possessed by correspondingly low types." "Higher biological types within the same order have certainly developed since in many cases, and probably higher mental functions, but some of Ordovician forms have suffered biological and probably also mental degeneration."[1]

Of instincts on this higher plane that show distinct genetic features, perhaps no modern forms that have been so well studied are more likely to be typical and suggestive of the cephalopods and gastropods that were typical of early Paleozoic times, than the modern fresh water snail, Physa studied by Dawson and allied forms by Cole (1) and Yung (10), very like the ancient gasteropods observed by Montague, Tait, Tye, Tryon, and Adams. Not only are there great individual differences, but young snails seem less sensitive than older ones, and act somewhat as the older ones do when tame. Those half-grown recover quicker than adults when equally disturbed. Very young snails rarely tap with their syphon when placed in a beaker directly from the pond, as older ones do, and their spontaneous and stimulated movements are simpler. Like all the helix forms of molluscan shells their type of growth suggests evolution and progressively "larger mansions for the soul" and body. Studied first over several hundred acres in Michigan, where they abound, they were found to be minutely adapted to their optimum conditions, having chosen marginal and littoral zones, temperature, shade, sedge, decaying debris, and to have provided for possible desiccation, for Physa does not burrow like bivals, and forms no epiphragm to withstand drought. As it creeps along the bottom it spins a ribbon of slime, and when

[1] T. C. Chamberlain's and R. D. Salisbury: Geology. Vol. II, p. 364. H. Holt and Co., New York, 1905-1906.

it needs to rise this is curled into a tube and anchored so that it can stop anywhere in its upward course. On reaching the surface film it turns its body about and its disk takes in oxygen. Here it feeds, and in going down after attaching to the surface it spins another line, which is at first transparent but which may be used several times even by other snails. Like other pulmonates it can rise or sink according as its lung is full or empty. Carmine grains show that these lines do not stretch, although they may be a foot or two long. . If it turns before reaching the surface it gathers in the line. It rises steadily like a toy balloon till within perhaps a centimeter of the surface and then accelerates, having a strange sense for a surface or for a solid which lies in its course before reaching it. It rarely spins a downward thread if its lung is empty. A hair may be accepted and used for a line of their own make. The tenacity of the mucus and of the spinning of its thread depends upon the intake of food, and it is less strong than the threads of air spinners, like spiders, which must sustain their weight. It seems hard for a well snail not to spin, but they cease to do so during hibernation or during cold or darkness. In flowing water they rarely spin but hug the bottom tighter to avoid being swept away. Fine gravel, stems and a rough but not precipitate bottom, which favors anchorage, increases the tendency to spin. Although they rise chiefly for air they do not always do so at the time of greatest respiratory need so that there are other, perhaps mechanical, impulses to that act. Physa is very sensitive to jars upon the surface even though it may lie some distance below it. This sense causes them to react on a tadpole or beetle that comes near them. When placed in deep water they always turn toward the shore and they rise and descend almost vertically, and seem to detect very slight angular difference from the line of gravity. When moving along the bottom, especially if it is an unaccustomed one, they project their syphons into long tentacles and tap along like a blind man with a cane. Wherever they are they at once seem at home as soon as they touch the surface film. The soil of a certain consistency in contact with the head seems to be stimulus for burrowing. It is important to observe the daily life in a native habitat before transference to aquaria, although snails readily adapt themselves to any conditions and can be tamed, and even accustomed to being handled. Other studies show that snails have considerable sense of direction. Land snails can retrace their course probably by a trail of slime for rods; seem attached to their mates and to localities. Closely allied forms react to odors (Sprengel), heat, light and even sound, taste (Nagel)[1] and others to geotactic stimuli in a way which

[1] Wilibald A. Nagel: Der Lichtsinn augenloser Thiere. G. Fischer, Jena, 1896.

is measurable (Davenport),[1] while intense light provokes, in Physa, great activity, with growing insensitiveness to mechanical influence and subsequent exhaustion (Pearl).[2]

As the snail and bivalve type of life of the Ordovician may be slightly inferred from their highest modern relatives or descendants, so the life of the great crustacea of the mid Silurian age which reached their culmination in the Eurypterus of the Devonian and carboniferous period may be, at least, to some extent approached from the study of modern crabs. The great restriction of shallow sea area in the Silurian era, which shows more species than any other during the tertiary and the adverse conditions attending the closing paleozoic period did not restrict their orthogenic development so that their pauperitic forms are few and this attests the vigor and the value of their structural and ecological paradigm of life. Rock crabs have keen and far sight, are timid and fly to their hole at the slightest alarm. They cannot live long under water and if they are frightened off a beach, dig themselves tight into the sand at the approach of every great wave. Land crabs travel by night in swarms, in May and June, and always in straight lines, even over houses to the shore, where they bathe a few times, lay their eggs in the water, and return to the mountains. One small species of crustacean builds conical nests of sea-weed where they rear their brood. During the moulting season, the common hard-shelled crabs are said to post sentinels to protect their comrades during their unprotected state. The lobster buries the uneaten part of its food in a heap of gravel and mounts guard. Certain crabs open cocoanuts by an ideal, invariable and the only way possible for them. Other aid anemones, which are their commensals, in attaching themselves to their shells. Even the young hermit crab, when he is first given shells or molluscs rushes to them, hesitates, mounts the mouth, and rides about until the tenant is dead, and then it tears it out and takes its place. Their love of change and activity impels them often to accept new and worse shells. In changing, the crab explores, passing from one to the other home, as if comparing by trial and use, and at last darts quickly into the new one. They pass through six metamorphoses before reaching adolescent[3] form, suggesting recapitulation of, so far, unexplored phyletic stages, and their bodies by this time soon become very asymmetrical, perhaps because they strongly prefer dextral shells. Yet young ones can adjust themselves

[1] C. B. Davenport: Experimental Morphology. Vol. I. The Macmilmillan Co., New York, 1897.

[2] R. Pearl: The Movements and Reactions of Fresh Water Planarians. Quart. Jour. Med. Sci., Vol. 46, pp. 509-714.

[3] See Thompson: Proc. Boston Soc. Nat. Hist., Sept., 1904.

to sinestral shells with less inconvenience than the old ones. Spaulding's[1] experiments (1904) on hermit crabs with the simple maze showed that they could profit by experience in association by vision and taste, and he believed that they could "reproduce, or if one will, vaguely remember." Yerkes[2] (1902) has shown that the crawfish can slowly acquire a simple labyrinth habit, and learn to turn always the right way to food when the chemical senses are excluded, some fifty to one hundred trials having established an almost invariably right choice of one out of two passages. He later showed that the green crab could be taught to go in a nearly straight line to its food instead of following a devious path as on its early trials. Dearborn (1900) found a little less individuality in the crawfish than he expected, but found them very susceptible to horrification and that touch was more important in their lives than sight.[3] Rotation produced no trace of vertigo. He measured their traction power and pinching strength. If they could not, when placed upon their backs turn over within five minutes, they gave it up. Most could be "hypnotized" by holding them still for about a minute when they became rigid, for ten or twenty minutes and aroused themselves suddenly. There was far greater variability in the conduct of each individual at different times than difference between one individual and another. Testing reaction time required painful stimuli, causing great psychic irritation. Inconsistency and absence of correlation between the results of the various tests was one of the last writer's conclusions.

Perhaps the first air-breathers were the upper silurian insects, the cockroaches and scorpions, the fossils of which show true spiracles, but it is certain that insect life had already begun upon land in this primeval age. All the Devonian insects which were rather abundant, as land vegetation was increasing, seemed to belong to the old net-winged order, some of them had chirping organs which of course implies hearing. It is amid the rank herbage of the carboniferous age, however, that insects appeared in great numbers and in many varieties, especially dragon flies, grasshoppers, cockroaches, spiders, scorpions, centipedes, etc., although flies, butterflies, bees, ants and social insects generally were still wanting. Honey-lovers had to wait the appearance of true flowering plants. Some insects of this age are found in vast numbers and among them occurs the largest insect known, the phasma, a foot long and with a 28 inch spread of wings. All the hexapod palæozoic insects are

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[1] E. G. Spaulding: Jour. of Comp. Neurol. and Psychol., March, 1904, Vol. 14, p. 49.

[2] Robert Yerkes: Habit Formation in the Green Crab. Carcinus granulatus. Biol. Bulletin, Oct., 1902, Vol. 3, pp. 241-244.

[3] For complete reference see p. 212.

highly generalized, some inclining to each of the three now widely separated orders, neuroptera, orthoptera and hemiptera. When we come to the Jura some 150 species are known in the trias alone, of which three-fourths are beetles; here, too, the higher groups begin. Even miocene vegetation was much more abundant than now, and more tropical, hence insect life was more plentiful and all orders including the highest are found. Some 2,000 fossil specimens having been discovered, some bits of sedimentary rock being black with their remains. In all Europe there are now 50 species of ants, but Heer found more than 100 species in a single miocene bed all winged. From this he inferred that loss of wings and the development of neuters came later in connection with the further evolution of social habits since the early tertiary. The miocene indusial limestone in France is a cement of cast off cases of the larva of the caddis fly. In several tertiary amber beds made of conifer resin over 800 species of insects have been perfectly preserved, and Scudder found 1,000 species including 7 out of the 16 known fossil species of butterfly in the Green River Shales.

Insects are now represented by many hundred thousand species and varieties, the oldest forms of which can be traced to the earliest geological age, and in them the marvels of instinct culminate. Insects live in the earth, air and water, construct intricate homes of many typical and purposeful forms, provide with amazing sagacity for the food and shelter of their young before the eggs are laid, by choosing a place within or upon animal bodies, plants, soil where they are sufficiently or best nourished, lay in the kind and amount of provision needed, devise many ways of preparing, preserving and accumulating food, and guarding it against robbers. They care for their eggs in elaborate ways, domesticating and rearing slaves and pets, seem to plant and harvest and cultivate fungus-gardens, gathering honey and pollen, secrete wax, silk, chitine, cellulose, poisons, develop many kinds of warning color, organize communities so elaborate that they are comparable with human social and political organizations, spin webs with vast mechanical difficulty and make balloons and float off on them, migrate great distances and in the utmost order, organize forays, fight pitched battles, carry on campaigns, have senses and modes of communication we cannot understand, show memory, fear and anger, migrate and hibernate, and seem to be hypnotizable, have intricate modes of reproduction, show memory both active and passive, have definite habits of individual cleanliness and of domestic and public hygiene, construct and set traps for their prey, besides their hard work have play spells, develop funeral habits, have a strange but usually unerring sense of direction, educate their young to pass through

distinct developmental stages after as well as before beginning their active life, construct almost mathematical cells, are masons, tapesters, carders, make diving bells, trap doors, have complicated customs of marriage flight and swarming, and do so much of the cross-fertilization of plants that most of our flowering species could not exist without them. From even such mere chapter heads of entomological psychology (to the detail study of which scores of the ablest minds of the present and recent past have given their lives and developed a vast literature in a field, which for its purely scientific as distinct from its highly economic value, university chairs should be established), the question arises and just in proportion as we read and observe, grows insistent whether if intelligence is adjustment and the accumulation of the results of experience, any other form of life, not excluding man himself, has attained any such perfection. Certainly none of the higher vertebrates can compare with insects in this respect. Their typical and communal activities and their individual ingenuity and inventiveness in meeting unusual emergencies are alike extraordinary, and the naturalist in this field meets with what Huber calls "brilliant flashes of reason." Experimental tests with insects show both surprising limitation to modes of response and also amazing cleverness in the original solutions of the problems presented. How could the ant, *e. g.*, have better met its conditions of life, structural and environmental, if it had from the first been endowed with human reason? Must we not conclude that psychic organization of the insect world is due to the fact of its age, for it is perhaps hundreds of times older than man? Its teeming populousness, too, has all this time made its social stimuli relatively greater than the environmental stimuli as compared with species with less individuals, so that mutual help has thus been more developed.

The above three types, not in man's line of descent, but which are far lower than even his animal pedigree and can be traced with much certainty, show not only great development of digestive, eliminative, and motor apparatus, respiratory, circulatory, reproductive, nervous and sensory mechanism and function, but cumulative and recapitulatory heredity, as well as stages of somatic and psychic evolution. The spiral and enlarging shell begins with the foramenifera, the nautilus, the only surviving representative of the once large and numerous tetrabranchiata, retiring from each chamber as it grows larger, and maintaining connection with all only by its siphuncule. The method of growth of the crustacea is by successive moults and that of insects by metamorphosis, the larvæ of the latter often showing more sagacity than the full imago. These three methods of development are all recapitulatory of the history of

the race, and have long been standing tropes of evolution to higher stages. Another conclusion forced upon us is that every tool, apparatus, muscle, structural part is certain to be utilized to the very uttermost. A carapace is strong and hard just where and to the degree that there is actual strain or attack. A claw or tentacle, eye, limb is used in every possible helpful way, and just as there cannot be any perfect living brain without a mind, and perhaps *vice versa*, so structure and use are one and inseparable. Every curve, hue, contour and size may be a matter of life and death, every mottle in the wing has a sufficient cause and each sportive variation not at once helpful tends to vanish. Prodigal and wasteful as she is of individuals, nature went through long and hard labor to produce types and so is very economical and careful of them. Even earlier forms that have gone the way of the lost cystoids of the Cambrian, the astrocods of the Silurian, and the ammonites and blastoids of the Devonian, and many others that seem to have vanished have, in fact, doubtless persisted in direct continuity in forms that varied beyond our power to trace them during the dark ages of transition, so that instead of dying, between the two successive but unconforming geologic periods, the old forms were transmuted beyond our power to recognize them. Precisely the same is true of other phyla. Many types of soul life have vanished or been transformed. Very likely vertebrate life would have seemed at this early stage aberrant or decadent, for its powers of adaptation to the manifold environments were probably far less than those of insects, which to the philosophical onlooker would have seemed the crown and entelechy of animal existence. A question we cannot answer is whether forms that were long ago more diverse, larger or abundant, also had in their early day a proportionately and higher development of instincts, so that their modern survivors have followed the law of once high and then decadent human stirps in repeating, in a conventional rudimentary way, conduct once far more evolved. This, however, would seem to harmonize with the mechanical nature of many responses, while the power to solve new and individual problems suggests that some instincts may be still in a process of formation. To my mind the most probable statement that can be made is that insects now do much that was learned in the early stages of their phyletic development, so that in observing them we are often face to face with processes far older than man or the present geological configuration of the globe. It is this momentum that makes animal instinct generally, and especially in its lower forms, so automatic. It need never have been discursive, but it is now chiefly innate, and in it is the condensed and impacted experience accumulated from an almost infinite number of generations,

the momentum of which impels nearly all the activity of these lives. It is thus impossible to account for them if the condensed results of experience are not, at least in some degree, inherited. Again, most insects with higher instincts are social, and a few now solitary were once so. A community is an instrument by which the aggregate develops the ability to profit by the smallest groups or even individuals within it. Not only division of labor, but the fact that each cell as it were of the body politic moves about, all of them combined over a wide area encountering divers chances, and then when they come together have some power of imparting and following their experiences, makes for manifold orientation or plasticity in the state. Higher animals are intelligent and domesticable, somewhat in proportion to their gregariousness in feral states, and ants and bees and other insects huddle and mutually impart their psychic states by contact as if they were parts of one body so intimate is their intercourse, yet each goes out by itself and accumulates impressions as it does food and honey for the common good. Thus they are "separate as fingers, but one as the hand." Observation and experiments show great difference among individuals and there are geniuses and fools in every large colony and swarm. Yet in the insect city the individual is more subordinate to the community than in any known higher form, hence, there is greater harmony with never a real ruler. Insects were, perhaps, the first forms of animal life to permanently emerge from the primeval sea, and if so they thus have the start of all others in adjusting to new and more complex situations. It is practically certain that they were the first to fly, adding thus a new and even physically larger field of experience.

The natural order of studies of life is first, classification and nomenclature, involving inner anatomical structure, then the study of life histories of individuals, a far more important and intricate work for which taxonomy is merely preparatory and of which we as yet know very little and that only of very few species. Even our acquaintance with terrestrial forms is limited, while of fresh-water and especially pelagic types we have but the slightest knowledge even of the species, the morphology of which is found in our text-books. Very likely many still unstudied are yet more remarkable than the highest yet known.[1]

[1] Daniels, Scudder, Lacoe for years collected fossil insects in this country and lately all those in our national museum were handed over to Professor Handlirsch, of Vienna, who found among them 137 new species, and has classified and located them so far as possible in the successive geological strata, especially those that belong to the coal measure and the Permian Age. Little is known of the horizontal distribution of these insects, but the similarity of the American to the

Thus of the life histories and the evolution of instincts, which ought to be the goal and the key of all, our ignorance is painful. Among the boldest and most important beginnings in this field are the attempts of a few pioneers like Buttel-Reepen, to construct the genesis of the social organization of bees and ants.[1] He thinks division of labor the *primum movens*. First the female hives alone, the male having vanished after the nuptial flight. The nest is of one cell and apart. Here the mother lives and dies alone before the young appear, each from its own hole in the earth. Next the mother finds it easier to give to several cells a common exit, and by thus economizing labor she can lay more eggs. This development was accelerated and the mother continued to lay longer till the first eggs hatch before she is done laying, and mother and offspring meet and the family begins. Some of the young do not fly out at once but help the mother build and provision the last cells. In parthogenetic species the young may even help the mother lay eggs. These individuals may never become fertilized so that they become sterile workers who stay at home while the males rove. Later, as now with wasps and bumblebees, the young female is fertilized in the fall and in the spring builds, and rears a brood, defending them and bringing food. Slowly the mother becomes only an egg layer and very fecund, while the workers lay no eggs, so that as the young and the queen are tended by the workers, each depends upon the other. This view shows how workers are derived from queens, which as opposed to current views that males vary more than any other members of a community. In some ants, besides the normal winged queen, there are often fruitful queens without wings. Workers are thus distinguished of different sizes and with different functions, and a soldier caste arose. Buttel-Reepen even attempts a genesis of the fungus garden of some ants. He found one queen which in several important respects repeated this genetic history, even living peacefully with another queen for a time, till the future colony was assured, then killing it. Quality or quantity of food, and perhaps both, aid in the fur-

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European species is very striking. As a result of comparing the number of forms in the single orders of the Paleozoic Age the paleodichyoptera seem to be the stem group on morphological grounds. It also appeared first and then steadily gave place to the seven more specialized orders, which latter are transitional to modern insects. These seven orders appear in their maximal numbers in the middle beds of the Paleozoic before giving place to the newer forms. Revision of American Paleozoic Insects, by T. R. Proceedings of the United States National Museum, Vol. 29, 1906, pp. 661-820.

[1] Die Stammesgeschichtliche Entstehung des Bienen Staats, Thieme, Leipzig, 1903. Also Wie entsteht die Amiesen-Kolonien. Arch. f. Gesellshafts Biologie, 1905, p. 20.

ther development, polymorphization and the division of labor. The evolution of the ant state, as he describes it, while following a somewhat different course, shows a more elastic organization than that of bees, including as it does aphids, etc., a greater independence of its members, more all-sided adaptation to food and forms of dwelling, and hence it has more power of persistence. Escherich who has studied ants for nearly half a century, thinks many phyletic features can be made out when we compare the over five thousand species and varieties of formicidæ, and holds that the workers with their larger brains have led the way. He believes they were winged at first and that those now so have reverted, Poulton suggests developmental stages for insect mimicry, and derives the wet phase of some dimorphic butterflies from an older dry phase, the two being often so different as to be thought distinct species. Wheeler[1] suggests a phylogeny of parasitisms and the results of mixing members of different nests and races, of slavery, etc. The data in all these cases are comparative and cannot be very firmly based on, or even connected with, paleontology.

For our part we believe the long period of inveterate and incessant disparagement of instinct and even of highly adaptive reflexes as unconscious and automatic and as lacking in the indefinite and very much over prized element of consciousness or discursive reason is passing, and that the future will see an opposite tendency, viz., that modes of life that fit the nature and needs of the species with exactness will be deemed high just in proportion as they have become blind and mechanical and therefore, securely established, and that man would tend to be morally and intellectually better if he did and thought rightly in all important matters as a music-box, when wound up, plays the right tune, with proper expression to the end, however long and complex it is. The superiority of instinct over reason is that it regulates conduct in the interest of the species at every point while consciousness is selfish and is exactly measured by the degree to which the individual has broken away from the dominance of the race and set up for himself against it. This is a disease of our infant race, but it is a grave one that threatens the very attainment of man's maturity. If our venerable animal forbears passed through such a stage of consciousness (as perhaps they did in various degrees when they were young as we are now) without becoming permanently aberrant, and reached their present perfect adjustment or else became extinct in exact proportion as they conformed to their law of life or swerved from it, then the chance of man's renormalization from the fall is perhaps about as grave as all religions make it.

[1] W. M. Wheeler: Nests of Ants. Am. Nat., July, 1903, Vol. 34, pp. 431-513.

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THE TRIDIMENSIONAL THEORY OF FEELING[1]

By E. B. Titchener

A lecturer who had expounded Wundt's elementary doctrine of feeling in the year of grace 1893 would have called attention to two principal points: the status of feeling in consciousness, and the number and nature of the affective qualities. Feeling, Wundt says in the fourth edition of the *Physiologische Psychologie*, is a third attribute of sensation, "eine dritte Eigenschaft der Empfindung." "Neben Intensität und Qualität begegnet uns mehr oder minder ausgeprägt in jeder Empfindung ein *drittes* Element . . . Wir nennen diesen dritten Bestandtheil der Empfindung den *Gefühlston* oder das *sinnliche Gefühl.*" And feeling or affective tone ranges between qualitative opposites, which "wir als *Lust-* und *Unlustgefühle* bezeichnen." Pleasantness and unpleasantness are the ultimate simple forms of sense-feeling; the irreducible qualities of the pure affective tone which is immanent in the simple sensation. At the same time, the terms 'pleasantness' and 'unpleasantness' are not adequate to describe the affective tone of any and every sensation that we obtain by psychological analysis. The qualities of the higher senses, sight and hearing, play an important part in the compound ideas which appeal to the æsthetic side of our nature. Probably for this reason, their affective coloring is approximately, *annähernd*, identical with that of such compound ideas; they have taken on a *Stimmungscharakter*, "der nicht mehr schlechthin auf Lust und Unlust zurückgeführt werden kann, sondern in andern, in gewissen Affecten deutlicher ausgeprägten Gegensätzen einen adäquateren Ausdruck findet." Tones, *e. g.*, may be grave or cheerful; colors may be calming or exciting. The passage from pure affective tone, pleasantness or unpleasantness, to these æsthetic, emotional shades of feeling may be traced through the series of the senses. Touch and the common sensations show pleasantness-unpleasantness with only a trace of "qualitative Färbung;" tastes and smells are predominantly pleasant or unpleasant, but nevertheless admit of "verschiedenartigere Gefuhlsfärbungen." Tones and colors, which are strongly pleasant or unpleasant to children and savages, have almost lost these attributes for the civilised adult,—though even for us the seriousness of deep

[1]A lecture delivered at Columbia University, Feb. 20, 1908.

tones and of black surfaces leans towards unpleasantness, and the excitement of high tones and of white towards pleasantness, —and have assumed an affective coloring whose general affinity to pleasantness-unpleasantness is, in extreme cases, proved only by its movement between qualitative opposites.

That, then, was Wundt's doctrine, taken at the purely descriptive level: sensations with an immanent attribute of pleasantness-unpleasantness, the original simplicity of which appears clearly enough in the lower sense-departments, but in the higher is obscured by æsthetic or quasi-æsthetic reference.

Now suppose that, as the novelists say, three years have elapsed, and that the same lecturer is discussing the same subject in 1896. He has in his hands the first edition of Wundt's *Grundriss der Psychologie*. And there he reads of "zwei Arten psychischer Elemente, die sich als Producte der psychologischen Analyse ergeben, . . . Empfindungselemente oder Empfindungen [und] Gefühlselemente oder einfache Gefühle." The constitutive attributes ("unerlässliche Bestimmungsstücke) of sensation are quality and intensity. Affection, too, possesses these attributes. But there is a difference. While sensible qualities are limited by maximal differences, affective qualities range between maximal opposites. While the number of sensible qualities is fixed by the differentiation of the sense-organ, the number of affective qualities is indefinitely large; for simple feelings are the subjective complements, not only of simple sensations, but also of compound ideas and of still more complicated ideational processes. And while sensations fall into a number of separate systems, there is but one affective system; tone and color, warmth and pressure are disparate, but "alle einfachen Gefühle bilden eine einzige zusammenhängende Mannigfaltigkeit, insofern es kein Gefühl gibt, von dem aus man nicht durch Zwischenstufen und Indifferenzzonen zu irgend einem andern Gefühle gelangen könnte."

Do, then, all these many affective elements fall within "dem allgemeinen Rahmen einfacher Lust und Unlust?" By no means! There are three *Hauptrichtungen der Gefühle*, three dimensional categories, "innerhalb deren unendlich viele einfache Qualitäten vorkommen." These are pleasantness-unpleasantness, excitement-inhibition or excitement-tranquillisation, and tension-relaxation. As a rule, Wundt says, psychologists have paid regard only to pleasantness and unpleasantness, and have relegated the other two affective classes to the emotions. But as emotions arise from the combination of feelings, the fundamental types of emotion must be preformed, *vorgebildet*, in the affective elements.

In cases like this, I always want to trace the motive. Like the lawyer in *David Copperfield*, I assume that in all such

cases there *is* a motive. What was it, then, that led Wundt to his change of opinion?

If my reading of Wundt is correct, the changes that he has made from time to time in his various systematic works have never been due, in any real way, to external causes, but have always represented the climax or culmination of a stage of internal development. The germs of the changes are invariably, I think, to be found in the prior Wundt, and the changes themselves are but the full and self-conscious maturity of ideas that had long been 'incubated,' had long been held in the obscure margin of consciousness. On the other hand, it is possible, at least in most cases, to point with a fair degree of probability to the external cause that brought these obscure ideas to the attentive focus. In the present instance, that external cause appears, very obviously, in the publication of Külpe's *Grundriss*. Let me be clear on this matter, even if I am repetitious! I believe that Wundt would have formulated his new affective theory in any event; the theory was implicit in him and in his previous writings. If Külpe had not given the touch that led to crystallisation, some one else would, sooner or later, have performed the same office. In fact, however, Külpe undoubtedly did furnish the external stimulus,—so that, indeed, we have to thank him, not only for his own *Grundriss*, but in a certain special and limited sense for Wundt's as well.

Let me take you, now, to the first edition of the *Physiologische Psychologie*, the edition of 1874. In general, the exposition is very like the exposition of 1893. But, in 1893, we are told that the affective tone of sensations of the higher senses is a *Stimmungscharakter*, a coloring that they have 'taken on' in virtue of their constant participation in æsthetic ideas. In 1874, the reference to æsthetics comes at the end of the discussion: the fact that sight and hearing have freed themselves of sense-pleasurableness and sense-unpleasurableness fits them to serve as elements in æsthetic effect. They are not grave and dignified and happy and cheerful because they have been æsthetically employed, but their gravity and cheerfulness are what enables us to employ them with æsthetic result. "Lust und Unlust," Wundt concludes, "sind, wie es scheint, nur die von der *Intensität* der Empfindung herrührenden Bestimmungen, während an die *Qualitäten* Gegensätze anderer Art geknüpft sind, welche zwar zuweilen in eine gewisse Analogie mit Lust und Unlust sich bringen lassen, an sich aber doch von diesen letzteren nicht berührt werden." Here is the doctrine of the plurality of affective dimensions plainer and more definite than it was twenty years later; here is, evidently enough, the germ of the doctrine of 1896.

Once more: the chapter from which I have been quoting is

entitled, in 1893, "Gefühlston der Empfindung,"—in 1874, "Sinnliche Gefühle." Is not that significant also? Affection, in 1874, is not an attribute of sensation; it appears in that rôle for the first time in 1880. Affection, in 1874, is a relation, the relation which sensation sustains to consciousness at large. "Als ein nach Qualität und Intensität bestimmter Zustand ist die Empfindung nur im Bewusstsein gegeben; in Wirklichkeit existirt sie daher auch immer nur in ihrer Beziehung zu demselben. Diese Beziehung nennen wir das sinnliche Gefühl."

Clearly, then, the whole of the new affective theory is implicit in the original edition of Wundt's great work. So far from suddenly reversing his attitude to affective processes, he has, in reality, returned to his first systematic position. In other words, the problem with regard to Wundt is not so much that he now makes affection an independent element with a plurality of dimensions and qualities, as rather that he ever did anything else. This problem, too, can be solved; but it is foreign to our present consideration.

We are to examine, in this Lecture, the theory which I briefly outlined a moment ago on the basis of the *Grundriss* of 1896. The theory has been widely and variously discussed, and I cannot attempt to cover the whole of the relevant 'literature.' I shall refer, for the most part, to the earliest statements of it, in the *Grundriss* of 1896 and the *Vorlesungen* of 1897, and to the latest systematic statement in the *Physiologische Psychologie* of 1902.

First of all, then, how does Wundt arrive at his three affective dimensions? How does he prove that there are three, and that these three are pleasantness-unpleasantness, excitement-inhibition, and tension-relaxation? Well! his main reliance is on his own introspection. Wundt is a man of keen sensibility. He writes of feeling *con amore:* he is fond of quoting Goethe's *Farbenlehre;* feeling has played a larger and larger part in his psychological system as time went on; as early as 1874, he had systematised, thrown into diagrammatic form, his affective reactions to colors and tones. So the new theory appears in the *Grundriss* without preface or apology,—"wird einfach als Tatsache eingeführt," Orth plaintively remarks,—takes its place in the exposition with all the assurance of established fact. Remembering its genesis, its deep rooted and slow growth in Wundt's mind, we need not be greatly surprised. Wundt had said in 1874: "Gelb. . . . regt an, blau stimmt herab;" and had emphasised "das eigenthümliche Gefühl des Aufmerkens" which appears "im Zustande des Besinnens oder der Spannung." No doubt, it seemed obvious to him in 1896 that the introspective evidence, though not expressed, would be understood,—if indeed the thought of expression ever oc-

curred to him. Now, after several years of criticism, he is more explicit; the *Physiologische Psychologie* introduces the theory by way of definite introspective analysis.

Even in the *Grundriss*, however, Wundt is not simply dogmatic. He explains (*a*) that a triple classification of the affective elements is required for the distinction of the fundamental types of emotion. Later on, it is true, he declares that a psychological classification of the emotions "nur auf die Qualität des Gefühlsinhaltes gegründet werden kann." The argument has a circular look; affections are classified by reference to emotion, emotions by reference to affection. I think, however, that it is formally sound. Theoretically, emotions may be classified by quality, by intensity, and by temporal course. In practice, intensity and temporal course fail to furnish reliable criteria: hence, emotions must be classified by quality. Qualitative analysis then reveals certain fundamental types of emotion, which must, of course, be preformed in the affective qualities. Emotive classification thus points us back to a particular classification of affections, while affective classification, to be adequate, must necessarily point forward to emotion. Formally, this reasoning is rather a matter of what Fechner would call the 'solidarity' of a system than an instance of merely circular argumentation. Whether it is materially sound is another question,—a question which Stumpf, *e. g.*, would answer with an emphatic negative.

Wundt also brings evidence of an objective sort, the evidence (*b*) derived from the method of expression. He lays but slight stress on pulse-correlation in the *Grundriss*: "es ist unzulässig die Ausdrucks- der Eindrucksmethode in Bezug auf ihren psychologischen Werth gleichzuordnen." In the *Vorlesungen*, too, the pulse-records are introduced to prove the physiological relationship of the 'lower' to the 'higher' feelings, some time before we reach the distinction of the three affective dimensions. It is not until 1900, in the *Bemerkungen zur Theorie der Gefühle*, that the changes in innervation of heart, vessels and respiratory mechanism — "ein überaus feines Reagens auf die leisesten Aenderungen der Stärke wie Richtung der Gefühle" — are given anything like an independent place in Wundt's argument. Do not fear, now, that I shall plunge you into the technical intricacies of the expressive method, and that the remainder of the hour will be filled with sphygmograph and plethysmograph, pneumograph and dynamograph! Even if that method came into our discussion, I could pass it over with the reminder that, not so long ago, I gave a critical review of it from this platform. But it does not come into our discussion. Grant everything that the most ardent disciples of the method demand, and then ask yourselves: where is the evidence, in these

correlations, that we are dealing with elementary mental processes? What have pulse-curves to say to the question of the irreducibility, the ultimateness in consciousness, of the experiences of excitement-inhibition, tension-relaxation? Wundt himself is careful, in psychological connection, to differentiate "specifische Beschaffenheit" and "elementare Natur." How can pulse and breathing be relied upon to make the same distinction?

Let us, then, dismiss the expressive method and come back to the *Grundriss*. Had Wundt stopped short at the point which we have now reached; had he stated his theory, shown its usefulness in systematic regard for the classification of emotions, and indicated the correlated differences in the pulse-tracings: his position would, I think, have been stronger than it actually is. But he attempts, further, (*c*) to connect the three dimensions of affection with the three relations in which a given feeling may stand to the temporal course of mental processes at large. Pleasantness and unpleasantness denote a determinate modification of our present mental state; excitement and inhibition exert a determinate influence upon the next succeeding state; and tension and relaxation are qualitatively determined by the preceding state. "Diese Bedingungen machen es zugleich wahrscheinlich, dass andere Hauptrichtungen der Gefühle nicht existiren."

And yet—quality is the criterion for the classification of emotions, and the classification of the emotions requires three ultimate affective dimensions! Here, surely, we have the fallacy of too many proofs. Wundt, it is true, offers in the *Bemerkungen* a defence of his dual argument. "Es handelt sich hier um Momente, die selbst wieder mit einander zusammenhängen" : "[es] kommt hier überall nicht ein Verhältniss von Ursachen und Wirkungen, sondern lediglich ein solches von Beziehungen und Bedingungen in Frage, die sich wenigstens vorläufig durch eine vollständige Analyse aus der Gesammtheit der complexen Bedingungen nicht isoliren lassen." If I understand these passages aright, Wundt's meaning is as follows. 'Consciousness is always exceedingly complex, so that the affective processes are given in complex relations and appear as variously conditioned. Causal analysis is, at present, beyond our powers. We can, however, trace certain relations and follow up certain part-conditions; and our results, different or even incompatible as they may look, are really abstractions from—represent moments of—a single system of causal interrelations. Hence, they may safely be set down side by side.' In the abstract all this may be granted. Still, however, I do not see, in the concrete, how the three affective dimensions can be guaranteed *both* by temporal relations to the course of con-

sciousness *and* by qualitative differences in emotion. The latter are enough, in themselves; the former is, at the best, a matter of reflection, of analysis above the elementary level; and its obvious superfluity tends to cast doubt upon the results of qualitative analysis proper, with which it is brought into agreement. For the rest, it is significant that, in his later writings, Wundt has dropped this principle of temporal relation as a means of affective classification.

In the *Vorlesungen* of 1897 a new principle makes its appearance. After distinguishing the three dimensions of pleasantness-unpleasantness, excitement-tranquillisation, tension-relaxation, Wundt says: "dass es noch andere Hauptrichtungen ausser diesen gebe, scheint mir nach der subjectiven Beobachtung nicht wahrscheinlich. Auch dürften die genannten den allgemeinsten Bedingungen entsprechen, unter denen Gefühle überhaupt entstehen." The dimensions are guaranteed first by introspection, and secondly (*d*) by the threefold character of affective conditions. The conditions are found in the "Empfindungs- und Vorstellungselemente, an die [die Gefühle] gebunden sind." Pleasantness-unpleasantness represent a quality-dimension; excitement-tranquillisation, an intensity-dimension; tension-relaxation, a time-dimension. "Die Bedeutung von Lust und Unlust als 'Qualitätsrichtungen' liegt darin, dass vorzugsweise in ihnen die Wirkungen der qualitativen Eigenschaften des gesammten Bewusstseinsinhalts zum Ausdruck kommen:" and similarly with the other two dimensions. Intrinsically, of course, every affection is a quality, qualitatively different from every other. But the affective qualities of the three dimensions reflect, express, are determined by the quality, intensity and temporal properties of sensations and ideas.

I am not here concerned with the correctness or incorrectness of Wundt's correlation. He has himself changed it, in the *Physiologische Psychologie* of 1902, where pleasantness-unpleasantness represent an intensive, and excitement-tranquillisation a qualitative dimension,—just the reverse of what was said in 1897. I am concerned with the correlation as a principle of classification. There are, Wundt declares, three general conditions of the arousal of feeling: the quality, the intensity and the temporal relations of conscious contents. And the threefold character of the conditions furnishes, along with introspection, evidence that there are but three dimensions of affection. What, then, has become of the spatial relations of conscious contents? The chapter-headings of the *Physiologische Psychologie* tell us that *Sinnesvorstellungen* are of three kinds: intensive, spatial, temporal. Spatial and temporal ideas may be grouped together as extensive; intensive ideas

differ from sensations by the composite nature of their intensity and quality. These intensive ideas are therefore responsible for two affective dimensions, the intensive and qualitative; the temporal ideas are responsible for a third dimension, the temporal; only the spatial ideas are excused from affective duty. I argue, then, in this way. In so far as affective classification is dependent upon the various forms of idea, in so far Wundt's classification is inadequate: for the spatial form of idea is as important, in the mental life, as the intensive or the temporal. And if there is no such thing as a spatial dimension of affective qualities, then we may justly doubt whether the principle of classification is sound, and whether any conclusion as to the number of affective dimensions may be deduced from it. Remember, I am not arguing on a matter of fact; I am disputing the application of a principle.

Wundt replies, in the *Bemerkungen*, that he has left spatial ideas out of account for two reasons: first, "weil sich mir Beziehungen derselben zu bestimmten Gefühlsrichtungen weder in der unmittelbaren subjectiven Beobachtung noch bei der Analyse der Ausdrucksbewegungen darboten;" and secondly, "weil es mir scheint, dass man sehr wohl bei jedem Affect qualitative, intensive und zeitliche Eigenschaften unterscheiden kann, während ich mit dem Ausdruck, der Zorn oder die Freude habe irgend eine räumliche Ausdehnung, keinen rechten Sinn zu verbinden weiss." The first of these arguments misses its mark for the reason that, in the *Vorlesungen*, the distinction of three general conditions of feeling, their connection with three forms of idea, is offered as *additional* evidence, over and above 'subjective Beobachtung,' for the finality of Wundt's classification. "*Auch* dürften die genannten Hauptrichtungen den allgemeinsten Bedingungen entsprechen unter denen Gefühle überhaupt entstehen." I object to Wundt that the one of his criteria is invalid, and he rejoins that the other is valid! The second argument goes equally wide. I did not assert that an emotion possesses spatial attributes, that an affection may be 'extended,' but that certain ideas possess spatial attributes and relations,—and that, if we are classifying affections by reference to the forms of ideas, then these spatial properties must be taken into account, as well as the intensive, qualitative and temporal. I use the phrase 'spatial dimension of affective qualities' precisely as Wundt uses the phrase 'temporal dimension,'—to signify affective qualities that are dependent upon ideational extension. I acquitted Wundt, just now, of the charge of circularity; I am afraid that I must here charge him with the logical error which is known in the vernacular as 'missing the point.'

In sum, therefore, Wundt's three affective dimensions are

supported, primarily, by his own introspection, while he has appealed, further, to the necessities of emotive classification; to the results of the method of expression; to the temporal relations of the affective processes; and to their general conditions in consciousness. The first of these arguments I take to be sound, both formally and materially, though I do not arrive by it at the conclusion which Wundt has reached. The second must be pronounced irrelevant; the third has been given up by Wundt himself; the fourth we have seen to be logically defective and psychologically indefensible.

We have now to consider the theory on the basis that remains for it: introspection of the simple sense-feelings and qualitative analysis of the emotions. I find a difficulty at the outset, in Wundt's terminology. You may have been surprised that when I have had occasion to mention Wundt's category of 'excitement,' I have paired it with 'inhibition' or 'tranquillisation,' rather than with the more usual term 'depression.' I have throughout been quoting Wundt's own words, but it is true that in the *Grundriss* 'depressing' is given as an alternative to 'tranquillising,' and that in the *Physiologische Psychologie* 'Depression' is suggested for the higher degrees of 'Beruhigung.' Wundt can, of course, do no more than take language as he finds it. But I think that his actual choice of words bears witness to a conflict, in his thought, between two purposes: the purpose of transcribing his introspections, and the purpose of maintaining the typical affective movement between opposites. Pleasantness and unpleasantness, *Lust and Unlust* are opposite in name, as well as in nature. What of *Spannung* and *Lösung?* In English, 'relaxation'—which, I suppose, is the nearest equivalent of *Lösung*—suggests rather the remitting or resolving of tension than its qualitative opposite: this latter would be better expressed by 'relief.' Possibly *Lösung* has for Wundt an implication of positive relief, of *Erleichterung*,—though it has not for me, nor for German friends of whom I have made enquiry. Wundt speaks also of the *Befriedigung*, the fulfillment, of expectation; but that term brings us perilously near to *Beruhigung*. The chief difficulty, however, arises in connection with the remaining dimension. What is the opposite of *Erregung?* Sometimes Wundt says *Hemmung*, sometimes he says *Beruhigung*, sometimes *Depression*. The antithesis *Erregung-Hemmung* comes from nerve physiology; *Erregung-Depression* comes, evidently, from observation of the emotions, normal and pathological; *Erregung-Beruhigung* appears to be the analogue of *Spannung-Lösung* and to convey the same suggestion. But what is in introspection the *felt* opposite of *Erregung?* I cannot myself identify the feelings of *Hemmung*, *Depression*, *Beruhigung;* I

cannot feel them as degrees of the same thing, as lying in the same affective dimension; I cannot always distinguish between *Beruhigung* and *Lösung*. *Erregung*, 'excitement,' seems to me to feel very differently in different contexts, to be an equivocal term. It is easy to say that such considerations are mere 'Wortklauberei,' but I am trying to express a real introspective difficulty.

If, then, I am to judge others by myself, this uncertainty in the meaning of terms may be at least a partial reason for the fact that Wundt's classification, despite its claim to finality, does not always command the assent even of those who agree with its spirit and intention. Gurewitsch, *e. g.*, in his *Theorie der sittlichen Gefühle*, makes a fourth affective category for *Streben-Widerstreben*. Vogt, again, ranges feelings of activity and passivity alongside of pleasantness-unpleasantness, arousal-depression, tension-relaxation. Wundt identifies *Strebungsgefühl* with *Thätigkeitsgfühl*, and makes it a total feeling, compounded of strain and excitement. Royce, on the other hand, is disposed to think that two dimensions—pleasantness-unpleasantness and restlessness-quiescence—are adequate to the facts of the affective life. I do not at all mean that these differences of opinion are fatal to the theory. But they testify—do they not?—to a lack of precise formulation. Royce throws two of Wundt's dimensions into one; Vogt and Gurewitsch split the same two into three.

The single dimension about which Wundt himself seems, from the first, to have felt no doubt is that of *Spannung-Lösung*. The other two dimensions, as I pointed out just now, have actually exchanged places in his system. And the same uncertainty characterises certain of his observations in detail. Let me give you an instance. In the Bemerkungen of 1900, Wundt writes: "ich wüsste . . . wenn ich vor die Wahl gestellt wäre, irgend einen dieser Eindrücke dem andern vorzuziehen, absolut nicht zu sagen, ob mir das rein spektrale Blau oder das Roth . . . angenehmer sei." This does not mean that the two colors would be equally pleasant. "Ich würde eben einem solchen Verlangen immer nur die Aussage gegenüberstellen können, dass diese Eindrücke an sich mit Lust und Unlust nichts zu thun haben." The passage is a little startling, when one remembers that work had already been done upon colors—and colors that were not spectral colors—by the method of impression! Two years later, now, we have the following: "wenn ich zuerst ein spektralreines leuchtendes Roth und dann ein ebensolches Blau im Dunkelraum betrachte, so kann ich nicht umhin, beide als im hohen Grad erfreuende, also lusterregende Eindrücke zu charakterisiren." True, the sentence is concessive; the next begins with a

'gleichwohl;' but it is, nevertheless, in flat contradiction to the former quotation. If two impressions are highly pleasant, they *can* be compared as regards pleasantness, and a judgment of greater, less or equal can be passed upon them. Similarly conflicting statements are made concerning high and low tones. I readily acknowledge, again, that these minor inconsistencies are in no sense fatal to the theory; indeed, Wundt has so often emphasised the importance for feeling of the "ganze Disposition des Bewusstseins" that I feel reluctant, as it were a morsel ashamed, to dwell upon them. Still, they are there! And it is not reassuring to find that the dimension *Spannung-Lösung* owes its exceptional position, the stability of which I spoke above, to its systematic connection with the doctrine of apperception. It must have occurred to many of you, when earlier in the Lecture I was arguing the claims of space as a condition of feeling in consciousness, to ask—what, then, after all, are the claims of time? Since, in the psychology of sensation, duration and extension are, both alike, to a very large extent equivalent to, interchangeable with intensity, why should they not be bracketed with intensity as the conditions of one and the same affective dimension? We should then have something like Royce's classification: pleasantness-unpleasantness, conditioned upon all the 'qualitative' attributes of sensation, and excitement-quiescence conditioned upon all the 'intensive.' Now Wundt recognises the equivalence, under certain circumstances, of intensity and duration. "Insbesondere kann die Lust-Unlustcomponente [bei längerer Einwirkung auf das Bewusstsein] ganz dieselben Veränderungen erfahren, die auch die Steigerung der Intensität mit sich führt." But feelings of *Spannung* and *Lösung* are "die specifischen, für die Aufmerksamkeitsvorgänge charakteristischen Elemente." "Da aber Apperception und Aufmerksamkeit zeitlich sich entwickelnde Vorgänge sind, die zugleich in einer bestimmten zeitlichen Folge wechseln, indem jede Lösung eine vorangegangene Spannung fordert, und eine neue Spannung wiederum nur auf Grund vorangegangener Lösungen einsetzt, so sind diese Gefühlscomponenten enger als die übrigen an den zeitlichen Ablauf der Bewusstseinsvorgänge gebunden." Any serious doubt, therefore, about Wundt's doctrine of attention and apperception must at the same time jeopardise this third dimension of simple feeling.

So far, I have spoken only of the three affective dimensions; I have said nothing of the multitude of elementary qualities which the dimensions are held to include. "Die qualitative Mannigfaltigkeit der einfachen Gefühle ist unabsehbar gross und jedenfalls viel grösser als die Mannigfaltigkeit der Empfindungen." So the *Grundriss*,—which proceeds to give two

reasons. First, every sensation of the multidimensional sensation-systems belongs to more than one affective dimension. Secondly and more importantly, the feelings that attach to sensation-complexes, intensive, spatial and temporal ideas, and to certain stages in the temporal course of emotion and volition, are nevertheless themselves irreducible, and must therefore be counted among the elementary affective processes. You will notice that these reasons are phrased in the language of a special psychological system, though the appeal to introspection is implied. Later on, the appeal becomes explicit; we are reminded that, *e. g.*, the feeling of gravity, *Ernst*, "in verschiedenen Fallen in seiner Qualität wieder variiren kann." In the *Vorlesungen*, the doctrine of the multiplicity of affective qualities follows naturally from the doctrine of the *Totalgefühl*. The *Physiologische Psychologie* relies upon an 'aufmerksame Selbstbeobachtung.' We are apt to overlook the great variety of the feelings, partly because they are intimately bound up with the objective contents of consciousness, partly because we have no words to express them. "Angesichts der [an der Hand des vergleichenden Verfahrens der Eindrucksmethode] ausgeführten Analyse scheint es mir in überwiegenden Masse wahrscheinlich, dass die sechs Grundformen . . . eben nur *Grundformen* sind, von denen jede einzelne eine sehr grosse Mannigfaltigkeit im ganzen verwandter, aber dabei doch von Fall zu Fall nuancirter Einzelgefühle unter sich begreift."

There can be no manner of doubt that, in this matter of the number of the affective qualities, the psychological pendulum has been swinging, of recent years, in the direction that Wundt has taken. Ladd emphatically repudiates the view that "'pleasure-pains' are exhaustive of the entire quality of the feeling-aspect of consciousness." The theory is simplicity itself: "but simplicity, in the interests chiefly of biological and experimental psychology, 'gone entirely mad.'" I do not know whether Ladd felt pleased or pained that he had written this last sentence, when two years later he read Wundt's *Grundriss*. He says himself, however, that "almost all mental states which are marked by strong feeling in the case of developed minds are *mixed* feelings." At any rate, he works resolutely through the sense-departments, in 1894, and makes out a long list of elementary processes. James, in the same year, remarks that "there are infinite shades and tones in the various emotional excitements, which are as distinct as sensations of color are, and of which one is quite at a loss to predicate either pleasant or painful quality." This position is, of course, entirely compatible with a dual view of *Lust-Unlust*, of "the primary *Gefühlston*:" indeed, the two doctrines seem to me to appear, side by side, in James' own exposition. Nevertheless,

the passage may fairly be cited in the present connection. Lipps, again, working as it were from the opposite pole to Wundt, has arrived, as we all know, at a very complicated classification of the feelings. Stumpf has expressed the opinion, as against Külpe, that "sinnliche Annehmlichkeit" and "sinnliche Unannehmlichkeit" cover "eine grössere Mannigfaltigkeit von Gefuhlsqualitäten." This array of convictions is imposing, even if there are authorities—Höffding, Külpe, Jodl, Ebbinghaus, Lehmann, Rehmke—upon the other side.

The fact is, of course, that the ultimate question of a previous Lecture, the question of the criteria of affection, has not been settled. The parties to the present controversy do not really 'feel' differently; but they approach the problem with a certain attitude towards affective process, with a certain general view of the status of feelings in consciousness. Ebbinghaus says outright that Wundt and Jodl, *e. g.*, are 'not talking of the same things.' Orth believes that Wundt's theory is the outcome "seiner ursprünglichen Auffassung des Verhältnisses zwischen Empfindung und Gefühl." Ladd writes with a sort of ethical, even religious, atmosphere upon him: how can you compare the pleasure of cheese and beer with the pleasure of seeing a good *Hamlet?* Lipps considers the feelings as modes of reference to the self; feelings are "Ichinhalte oder Ichqualitäten." Stumpf adopts a sensationalist view of the sense-feelings; and in sensation qualitative differentiation is obvious enough. James is concerned with the varieties of emotive experience, and his protest against the 'hackneyed psychological doctrine' that pleasure and pain are the essence of emotion comports, as I have pointed out, with a strictly dualistic view of the affective qualities proper. It is not that our affective experience is radically different, but that we approach it from different directions, see it under different angles, assimilate it in terms of our systematic associations.

I do not mean that the point at issue is a mere *Etikettenfrage*. It is much more than that. Our decision 'makes a difference,' as the pragmatists say, to the whole structure of our psychological system. And it must be remembered that Wundt does not acknowledge any other methods than those employed by the dualists, and would not acquiesce in the statement that his results are of another order. He comes within our universe of discourse; he invites argument. I therefore proceed to argue: and I take as ground for argument an illustration which he employs on more than one occasion,—the feeling which attaches to the common chord *c-e-g*.

Let me remind you, first, of Wundt's doctrine of the *Totalgefühl*. A compound feeling, a feeling due to the confluence of a number of elementary feelings, is always psychologically

simple in the sense that it has its own irreducible quality, but may also permit the distinction of its various components. "In jedem derartigen Gefühl lassen sich *Gefühlscomponenten* und eine *Gefühlsresultante* unterscheiden." The components Wundt terms 'partial feelings,' the resultant, 'total feeling:' we have had an instance already in the 'feeling of activity' which results from the compounding of tension and excitement. The compound feeling thus bears a close resemblance to the formation which, in the sphere of tonal sensation, is called a fusion; Wundt speaks, in the *Physiologische Psychologie* of 'affective fusions.' There are degrees of affective, as there are degrees of tonal fusion; the partial feelings may appear simply as an undifferentiated coloring of the resultant, or may maintain their individuality, though in a subordinate position, alongside of the total feeling.

After this preface, we are ready to listen to the three tones. To prevent a swamping of the partial feelings by the total feeling,—the highest degree of affective fusion,—we take the tones separately in succession, and observe how they 'feel' in isolation. The tone *c*, heard by itself, affects us, Wundt says, by way of a 'calm seriousness' or a 'quiet cheerfulness;' it brings out feelings of two dimensions, pleasantness-unpleasantness and excitement-tranquillisation. The other two, *e* and *g*, will do the same,—though the affective qualities will be somewhat different. If, now, we put the tones together in pairs, every pair will give us a compound feeling: we have the three total feelings of *ce*, *eg*, *cg*, accompanied or colored by the partial feelings which we have compounded. And if the conditions are favorable for observation, we should be able to distinguish a fivefold feeling in connection with every pair; the two dimensions of the two partial feelings, and the total feeling. Now let us sound all three tones simultaneously. We have the total feeling of *c-e-g;* we have three relative total feelings, or 'partials of the second order,' as Wundt calls them,—the feelings of *ce*, *eg*, *cg;* and we have the 'partial feelings of the first order,' the six elementary feelings aroused by *c*, *e* and *g*. The feeling of *c-e-g* is a tenfold complex. Do not forget that such a feeling is, for Wundt, an "einheitliche Mannigfaltigkeit;" do not forget that the partial feelings may, more or less completely, have forfeited their independence. But, with all allowance made, ask yourselves if you experience anything like the body of feeling that, on Wundt's theory, you 'ought' to experience. Suppose that, in spite of our precautions, affective fusion has reached its highest degree; let the partials of the first order disappear altogether, as separate components, and let them remain only as a vague coloring of the whole affective impression. Now your compound feeling should be a

fourfold complex. Surely, it is not: surely, the feeling lacks the depth, the solidity, that a feeling thus compounded must possess: surely, you can describe the chord in no other terms than 'slightly pleasant,' 'moderately agreeable.'

I think that it is fair to test the theory in this way, by the judgment of a group of psychologically trained observers, seeing that Wundt has laid the observation before the psychological public in two of his books. I have, for myself, repeated the test often and again, and have varied it in half a dozen ways: always, while the chord remains a single impression, a sensible fusion out of musical setting and so far as possible freed from musical significance, I get the same meagre affective results.

If, now, Wundt retorts that in this and like instances we are feeling-deaf and feeling-blind, may we not suggest, on our side, that he is organically anæsthetic? The lack of interest that Wundt shows in the organic sensations has always been a source of wonderment to me. Take the new edition of the *Physiologische Psychologie*. Here is a total of 2,035 pages. Of these 45 are given to *Tast- und Gemeinempfindungen;* the *Gemeinempfindungen* alone, which I now have principally in mind, receive four, two and a half of which are devoted to pain. Of course, there are all sorts of scattered references. But look in the index under *Organempfindungen*, *Gemeinempfindungen*, *Niedere Sinne*, *Gelenkempfindungen*, *Muskelsinn*,— what you can think of. Aside from *Bewegungsempfindungen* and *Augenbewegungen* there is surprisingly little. Meumann makes a similar complaint with regard to Nagel's *Handbuch*. "Vermisst hat der Referent, dass den inneren Empfindungen (Organempfindungen) kein ausführlicheres Kapitel gewidmet wird; die gegenwärtige Physiologie scheint sich mit der Frage der Sensibilität der inneren Organe nicht mehr viel zu beschäftigen." Now I personally believe that the organic sensations play an important part, not only in feeling and emotion, but in many other departments of the mental life: in the formation of sensory judgments, in the mechanism of memory and recognition, in motives to action, in the primary perception of the self. It is true that, as compared with what we know of sight and hearing, our knowledge of the organic sensations is scrappy in form and small in amount; that is why I have said, in another connection, that "of all problems in the psychology of sense which are now before us, the problem of the nature, number and laws of connection of the organic sensations appears to me to be the most pressing." Let me add, now, that if any one of you is thinking of a piece of work in this general field, he would do far better, in my opinion, to start out from the side of the organic sensations than to succumb to the fascinations of pneumograph and sphygmograph.

Well! I believe that organic sensations are responsible for the dimensions of excitement-depression and tension-relaxation. On this point I can claim the support of Ebbinghaus and, I suppose, of all those who accept the James-Lange theory of emotion.

Stumpf, too, declares that he cannot regard them as "Elementarerscheinungen," though he offers no further analysis. But I believe, also, that organic sensations are responsible in certain cases for a *Nuancirung*, a shading and coloring, of feelings in the dimension of pleasant-unpleasantness. I say 'in certain cases,' for two reasons. First, it is entirely possible that this *Nuancirung* is a matter, not of simple sense-feeling, but of association, of emotive residua. Secondly, however, I do not think that the coloring and shading is as universal as Wundt asserts. Vogt, whose method of suggestion led him to the distinction of four pairs of feelings, is unable to discover it. Orth cannot find it, in the introspections that he educes by the *Reizmethode*. Störring's observers, on the other hand, report a qualitative difference between *Stimmungslust* and *Empfindungslust;* but though this is, so to say, a gross difference, the expressions used are singularly disappointing. We read, in some detail, of extensive differences, differences in intensive fluctuation, differences of excitement and passivity; but on the side of quality we have only "Stimmungslust ist gleichartiger," and the dogmatic statement "Zwischen Stimmungslust und Empfindungslust besteht *qualitative* Differenz." I, myself, have never observed a qualitative differentiation of pleasantness-unpleasantness, under experimental conditions; and when I observe a difference in everyday life,—a difference on the level of the sense-feeling,—I seem to find a reason for it in concomitant organic sensations.

I have sought, on two occasions, to put Wundt's theory to an experimental test. The method employed was the method of impression, in Cohn's form of paired comparisons. The procedure, in brief, is as follows. A series of stimuli—tones or colors or rhythms—is laid out, and the stimuli are presented to the observer two at a time, care being taken that every member of the series is paired with every other member. The observer has to decide which of the two stimuli shown him is the more pleasant, the more unpleasant, the more exciting, the more depressing, and so on. If colors are exhibited, he points to right or left, as the case may be; if tones are used, he notes down '1' or '2,' according as the first or the second stimulus is preferred. The work is laborious, and the method consumes a large amount of time. We have, however, the great advantage of a twofold control, objective and subjective.

The subjective control is afforded, of course, by the intro-

spection of the observers. The introspective task is extremely simple; the observer has merely to be passive, to let himself go, to allow the stimuli to take affective possession of him; and then to indicate, in the particular instance, which of the two makes the stronger impression. Moreover, since the introspective experience within a series is cumulative, all of the same kind, the observer is able, in the intervals between successive series, to give a general account of his method of judgment, of the nature of his affective reaction. The objective control is afforded by the course of the affective judgments themselves. If, *e. g.*, pleasantness and unpleasantness are really affective opposites, then the 'curves' or tracings which indicate the distribution of judgments in parallel, 'pleasant' and 'unpleasant' series, should be diametrically opposed: a color which stands high on the scale of pleasantness should stand low on the scale of unpleasantness, and contrariwise. If excitement-depression and tension-relaxation also denote affective opposites, then their 'curves' should be similarly opposed.

The stimuli chosen were colors, musical tones, and groups of metronome beats given at varying rates. The two former had been specified by Wundt as productive of excitement-depression, the latter as productive of tension-relaxation. My idea was, on the subjective side, to test by their means the immediacy of reaction in these dimensions. In the case of pleasantness-unpleasantness, you cannot say what the basis of your judgment is, otherwise than that it resides in the stimulus; the one of two colors or two tones *is* more pleasant than the other, just as directly as it is bluer or louder. Suppose, then, that colors and tones bring out equally prompt and unmediated judgments of excitement-depression, and that metronome intervals bring out equally prompt and unmediated judgments of tension-relaxation: then we shall have some ground for the acceptance of the two new affective dimensions. Suppose, on the other hand, that the judgments of excitement and tension are forced or difficult, mediated by associations or by organic sensations: then we shall have an introspective differentiation of these judgments from those of pleasantness-unpleasantness.

On the objective side, I argued in much the same way. Suppose that the curves, of which I spoke just now, show typical differences,—so that the distribution of judgments of pleasantness takes one course, that of judgments of excitement another, and that of judgments of tension a third,—while still the curves of pleasantness and unpleasantness, of excitement and depression, and of tension and relaxation are related as opposites: then, again, there will be ground for the acceptance

of Wundt's dimensions. Suppose, on the contrary, that the curves of excitement and of relaxation agree with the curve of pleasantness, and the curves of depression and of tension with the curve of unpleasantness: then, since the pleasant-unpleasant dimension is not in dispute, we have a strong indication that that alone is fundamental, and that the other two dimensions are affective only because and in so far as pleasantness and unpleasantness are involved in them.

The results of the first investigation, in which colors and musical tones were tested for pleasantness-unpleasantness and excitement-depression, and metronome intervals for pleasantness-unpleasantness and tension-relaxation, were published in the Wundt *Festschrift;* those of the second, in which the same tones and intervals were tested for all three of the Wundtian dimensions, were published by Hayes in the *American Journal of Psychology*. They may be summed up under three headings.

(1) Judgments of pleasantness and unpleasantness are direct, easy and natural. The qualities themselves appear to the observers to be simple and homogeneous, identical throughout the experiments. Their opposite character is vouched for both by introspection and by the course of the curves.

(2) Judgments of excitement are less direct, and the term is equivocal. If it is taken as the opposite of depressing melancholy, its curve agrees with that of pleasantness; if it is taken as the opposite of tranquility or soothing calm, its curve agrees with that of unpleasantness: the reverse curves then agree with those of unpleasantness and of pleasantness, respectively. If, in default of special instruction, the observer vacillates between the two meanings of the word, the curve shows a vacillating character, partly 'pleasant' and partly 'unpleasant:' the period and nature of the affective oscillation are vouched for by introspection. Judgments of depression are, in their turn, distinctly less direct than those of excitement, and are often associatively mediated. There is no evidence of a dimension of excitement-depression, and none of a number of exciting and depressing qualities.

(3) Judgments of tension are easy, but tension is described throughout in kinæsthetic terms. Increasing tension means, uniformly, increasing unpleasantness, and the curves of the two classes of judgment correspond. Relaxation may be taken as the opposite of unpleasant tension, in which case its curve agrees with the curve of pleasantness, or may be identified with depression. Nowhere is there evidence, in this third case either, of a new affective dimension or of specific qualities.

Of course, these results are not 'conclusive.' For one thing, the experiments are too few. For another, they were obtained in a single laboratory, and that a laboratory from which criti-

cism of Wundt's doctrine had already proceeded. For a third, the argument upon which the experiments rest is not demonstrably valid. It would, I think, be a very strange thing if three sets of stimuli should affect a number of observers by way of excitement-depression (or tension-relaxation) precisely as they do by way of pleasantness-unpleasantness,—but nobody can prove that such a state of affairs is, on the plural theory, impossible. Were I a champion of affective plurality, I should unhesitatingly urge these objections to the work, and I have no desire to slur them over because I am on the other side. Nevertheless, the results are experimental evidence: Wundt cannot, in the future, appeal to the method of impression as confidently as he has appealed in the past. And if our investigations are compared with those of Brahn and Gent, upon which Wundt relies in the *Physiologische Psychologie*, it will appear, I am very sure, that the critical sauce meted out to the goose must be considerably strengthened for the gander.

If now, in conclusion, I may give, with all due modesty, my own reading of the situation, it is this: that Wundt's tridimensional theory of feelings shows, as it were in typical form, the peculiar features that distinguish his psychology at large. Wundt has, in an eminent degree, the power of generalisation, and his generalisations cover—as generalisations oftentimes do not—an encyclopædic range of detailed knowledge. But the exercise of this very power leads him to put a certain stamp of finality upon his theories, as if questions were settled in the act of systematisation. You know what I am thinking of: the theory of space perception, the theory of attention, the definition and demarcation of psychology itself. The affective theory which we have been discussing is typical, then, both for good and for bad. It is good, in that it gives rounded and complete expression to a psychological tendency that, in many minds, has been struggling for utterance. It is bad, in that it offers a solution, ready made, of problems which in actual fact are ripe only for preliminary and tentative discussion. Like those other theories of attention and of space perception, it represents the culmination of an epoch of psychological thought; but, like them again, it is rather the starting point for further enquiry than the statement of assured psychological result. On the whole, I take it as matter of encouragement that generalisation has been at all possible. What has been done, provisionally, at a lower level of knowledge, can be done again, and better done, at a higher. In the meantime, we must not be dogmatic, we must not be too impatient for results, we must not set theory above observed fact: recognising to the full the difficulty and the merit of constructive effort, we must use all the weapons in our critical armory against ourselves as against others, and against others as against ourselves.

THE DAILY LIFE OF AMŒBA PROTEUS

By David Gibbs, Ph. D., in collaboration with Dr. O. P. Dellinger

From the Biological Laboratory of Clark University

The purpose of the investigation described in this paper was to determine the events in the daily life of *Amœba proteus*—its periods of rest and activity, its reactions to foods and other natural stimuli in its surroundings, how it lives and what it does.

The investigation was made by watching continuously for six days and five nights several amœbas and keeping careful records of their activities. One amœba was followed with special care, while several others under various conditions as to food were also observed continuously during this period, and daily for several weeks following. The amœbas were kept under as nearly natural conditions as possible, and there was every indication that their lives under the microscope were normal. The investigation was made in the Biological Laboratory of Clark University during the winter of 1905-6, under the direction of Professor Hodge. The continuous observations were maintained in relays by Drs. Hodge, Dellinger and the writer.

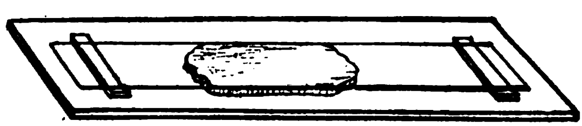
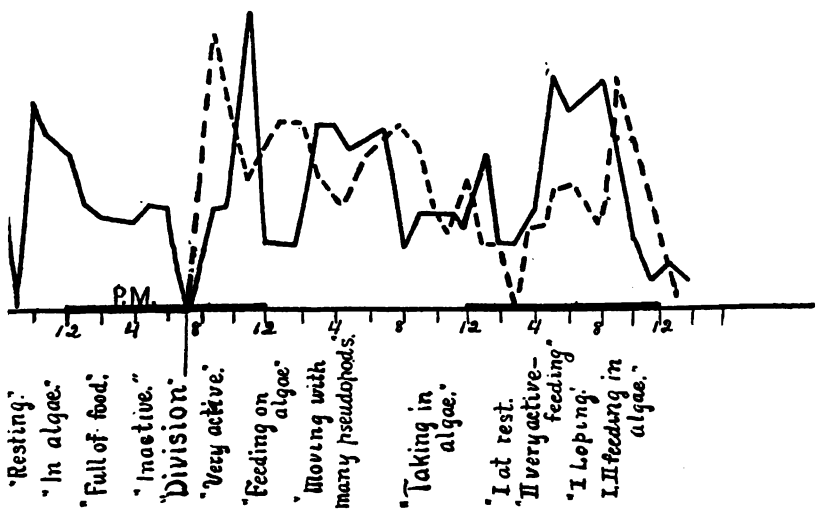


Fig 1.

The amœbas were kept in a special slide, consisting of a long cover-glass resting on two glass strips cut from an ordinary slide. A drop of water containing the amœba, when inserted between the slide and the cover-glass, clung to both by capillary attraction and made a perfect cell, open to the air, admitting freely of the insertion of pipette, needle, or additional water or food. The cells, when not under observation, were placed in a covered glass jar to prevent evaporation. The amœbas in one cell in this way were kept under daily observation during a period of eight weeks.

The question of the behavior of amœba is not a new one. The movements of no one animal have been studied so repeat-



e without pseudopods.

edly, so carefully, for so many years, and so frequently referred to as those of the amœba. College courses in biology, zoölogy, and some important branches of these sciences find it convenient to start with the amœba. The amœba is basal in biological and physiological theory as a key to the functions and general activities of protoplasm. "Amœbic" and "amœboid" are common terms in scientific literature. The amœba figures in discussions of immortality, heredity, and death. Further, because of the amœbe's apparently simple structure, attempts to produce life artificially have been largely in imitation of the amœba, and it has been connected with "primordial slime" as probably the first animal to be developed. All these indicate how important, even fundamental, is the life history of this animal for the sciences concerned with the theory and development of life.

Throughout the animal series in general the activities of *search for food* and the *rhythm of work and rest* are basal. Success in search determines the life of the individual and ot the species to a very large extent. Search is illustrated in all degrees in the life series from the gradual explorations of the root tips of plants to the complex activities of man. The rhythm of work and rest is a necessity imposed by the nature of protoplasm. It is illustrated in all degrees of rest from mere cessation of activity to profound slumber and hibernation. This rhythm is closely connected with effort in search and the attainment of food. For the higher animals this rhythm is necessary for life, but it has been doubted whether it is necessary or is exhibited in the life of the protozoa. The answer to this question is very important in its physiological bearings.

The investigation here briefly summarized showed very definitely that *amœba proteus* does have periods of activity and of rest as reactions connected with search for, and attainment of food. These periods apparently have nothing to do with light or darkness, day or night. The amœba moves actively feeding until well filled with food when it remains quiescent for a time.

These general facts are shown by the curve opposite, which represents the measured activity of an amœba during four days and nights, up to division, and the activity of the parts for two days and nights thereafter.

The curve shows the amount of movement. During the observations the movements of the amœba were measured by the micrometer gauge. At the same time careful notes were taken of the activities of the amœba. Quotations from these notes are placed below the curve and serve largely to explain it. Together they show fairly clearly that a period of activity and feeding was followed by a period of cessation of general move-

ment, and often of apparently complete rest, that the greatest activity was immediately following a period of rest, that the degree and length of rest were in proportion to the degree and length of activity, that the rhythm of activity and rest was most pronounced during the twenty-four hours before division, that during the twelve hours before division the amœba was less active, and divided when at rest, that the parts immediately after division were very active and maintained a high level of activity during the two days in which they were observed.

The greater activity of the parts after division is also definitely shown in the following cuts which represent the actual paths travelled by, first, the amœba before division, and, second, by the parts after division.

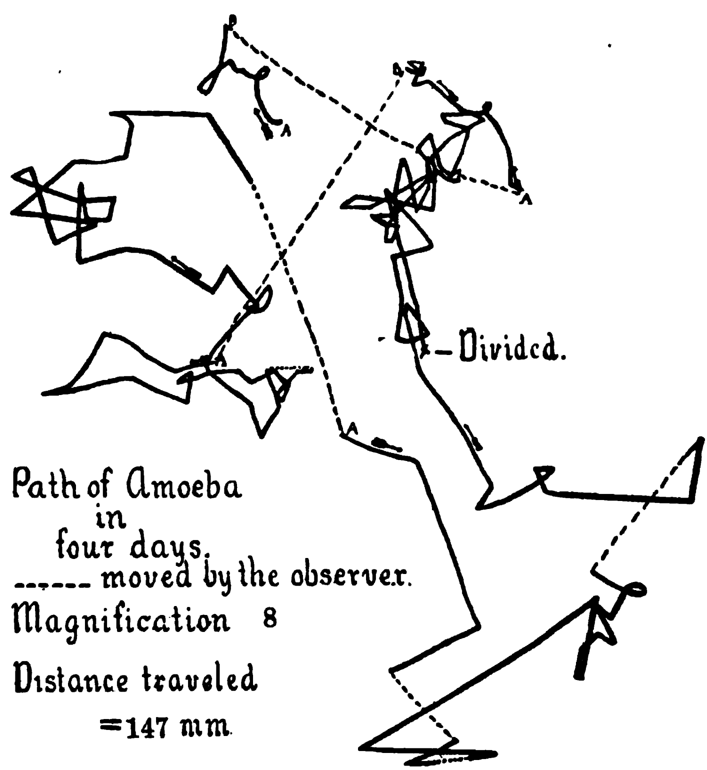


FIG. 3.

This diagram (Fig. 3) shows that the amœba travelled 147 millimeters during the ninety-six hours preceding division, while the following diagram shows that after division one part travelled 45 mm., and the other 52 mm. in thirty-one hours.

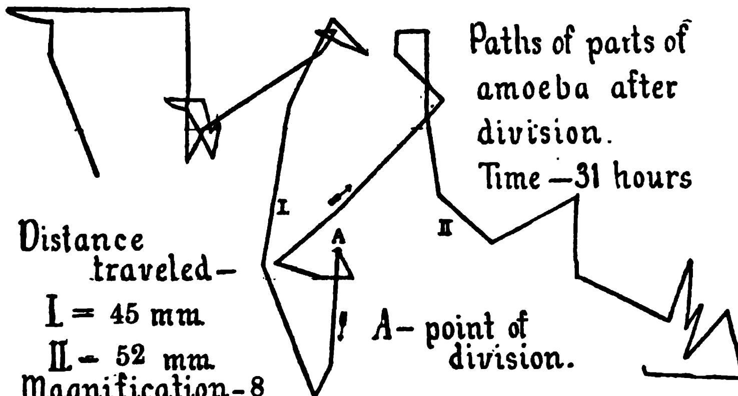


FIG. 4.

The amount of activity is also related to the amount and the kind of food. The amœba observed above was feeding mainly on unicellular algæ and was disturbed only occasionally by ciliates. When feeding on algæ scattered in the field, the amœba was more active than when feeding in a field of abundant supply. In either case it was necessary, however, for the amœba to move about to get its food.

On the other hand, amœbas, which were feeding on ciliates, when these were numerous, moved comparatively little and at long intervals of time, but when the ciliates were less numerous the amœbas were again more active.

The following curves (Fig. 5) show the activities of two amœbas feeding on ciliates. They were observed continuously for sixty-six hours. In the first place it will be observed that the activity was greatest during the first part of this period and became less and less. This was apparently the result of an increase in the number of ciliates in the cells. The necessity for action became less and less. A comparison of this curve with the curve of activity above (Fig. 2), which is on the same scale, shows strikingly this relation of movement to the food. The same fact is also even more strongly illustrated by a comparison of the paths of the amœbas in the two cases.

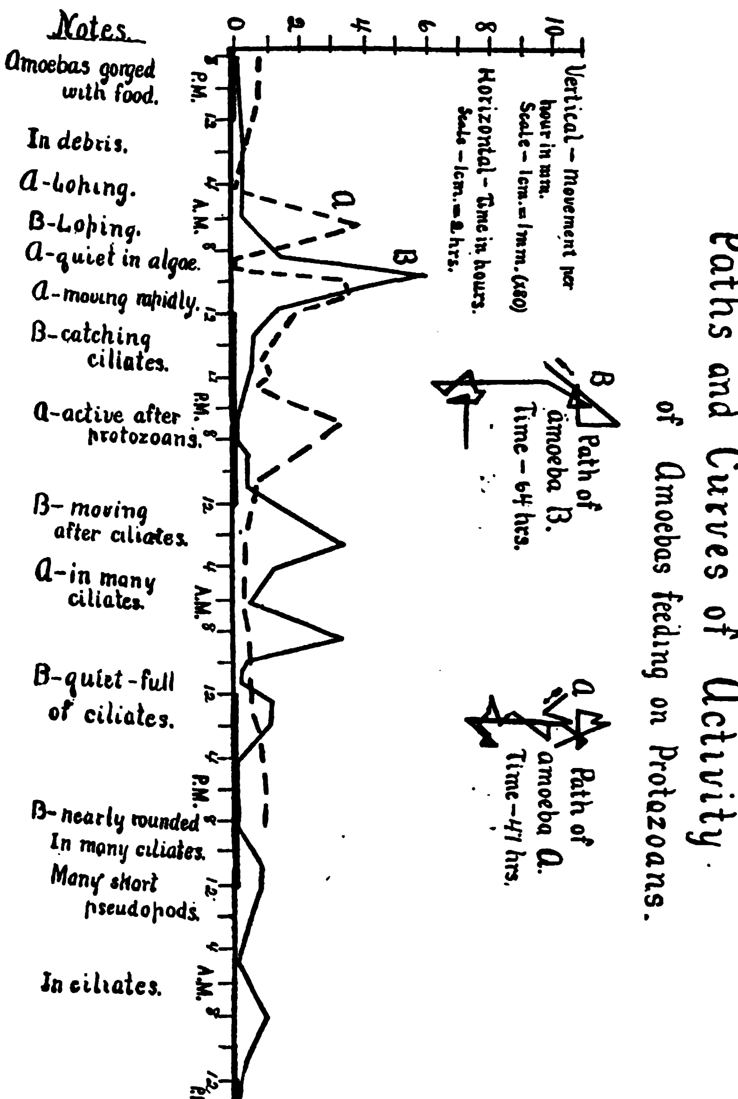


FIG. 5.

A similar investigation made in the Biological Laboratory of Clark University on *Vorticella* seemed to show that this animal worked continuously, without any periods of rest. From this, others (not the investigators) have concluded that proto-

PLATE I. An amœba feeding on algæ.

PLATE II. An amœba feeding on protozoa.

zoans never rest and that the rhythm of work and rest was only gradually evolved with the more complex forms of life. It seems, however, that the rhythm is here well developed in this lowest form of animal life, Activity, feeding, rest; activity, feeding, rest, is the story of the above curve. Activity, or the performing of work, requires energy which the protoplasm must supply. The period of rest appears to be simply the result of organic satisfaction, or a period of recuperation. It suggests the lowest form of sleep; for this tendency to rest, to sleep, as a food reaction is illustrated by the higher animals. In this respect this lowest form of life does not differ essentially from the higher forms.

The reactions of the amœba to, and its search for, food also brought out many important facts in its life history.

In the absence of food the amœba often moved by "lopes", that is, by lengthening and shortening its body as a whole without extra pseudopods, and moving rapidly forward.

Amœbas feeding habitually on algæ assumed a palmate form, moving forward with many pseudopods, similar to the form pictured in Plate I.

An amœba feeding on ciliates responds to a touch by a ciliate by sending out pseudopods at the point touched and following up the ciliate. The pseudopods are sent out on both sides of the ciliate until the latter is nearly enclosed and then rapidly connected, forming a chamber, in which the ciliate may finally be engulfed. The ciliate may, however, escape, leaving the chamber open. Some observers, seeing the "pursuit" at this stage, have concluded that the amœba formed the chamber before the partial enclosure of the ciliate.

In the presence of a large number of ciliates the amœba moves about but little. It sends out many pseudopods forming pockets, apparently in readiness for them, and often succeeds in catching them with remarkable rapidity. Our observations seem to indicate that this reaction is characteristic only of amœbas that have for several days been feeding on ciliates.

The adjacent photograph (Plate II) shows an amœba catching protozoa. The following series of camera lucida drawings of an amœba moving into a field containing ciliates shows the forms characteristic in a number of captures and also the peculiar pocketed outline and hooked pseudopods which seemed to be characteristic of forms feeding habitually on ciliates. See Fig. 6.

The amœba often follows a paramœcium or a ciliate until it is caught or lost. This "pursuit" may continue for twenty minutes or more as is indicated by the drawings of an actual instance, Fig. 7. The drawings show the touch by the paramœcium, the "pursuit," the partial capture and formation

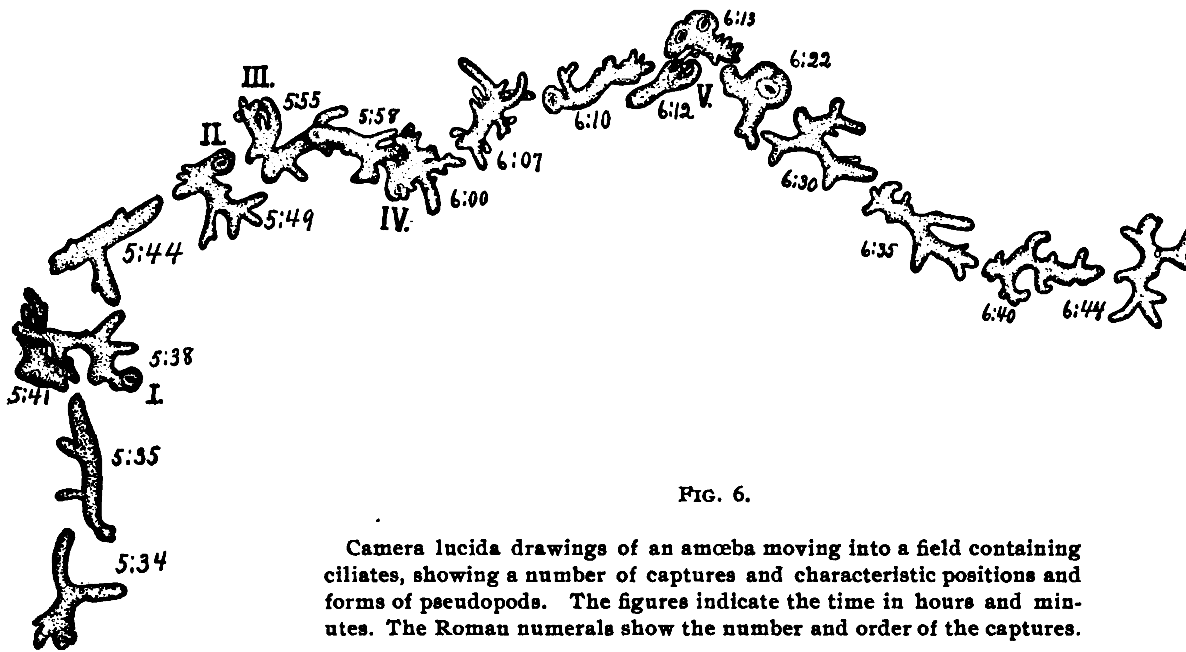


FIG. 6.

Camera lucida drawings of an amœba moving into a field containing ciliates, showing a number of captures and characteristic positions and forms of pseudopods. The figures indicate the time in hours and minutes. The Roman numerals show the number and order of the captures.

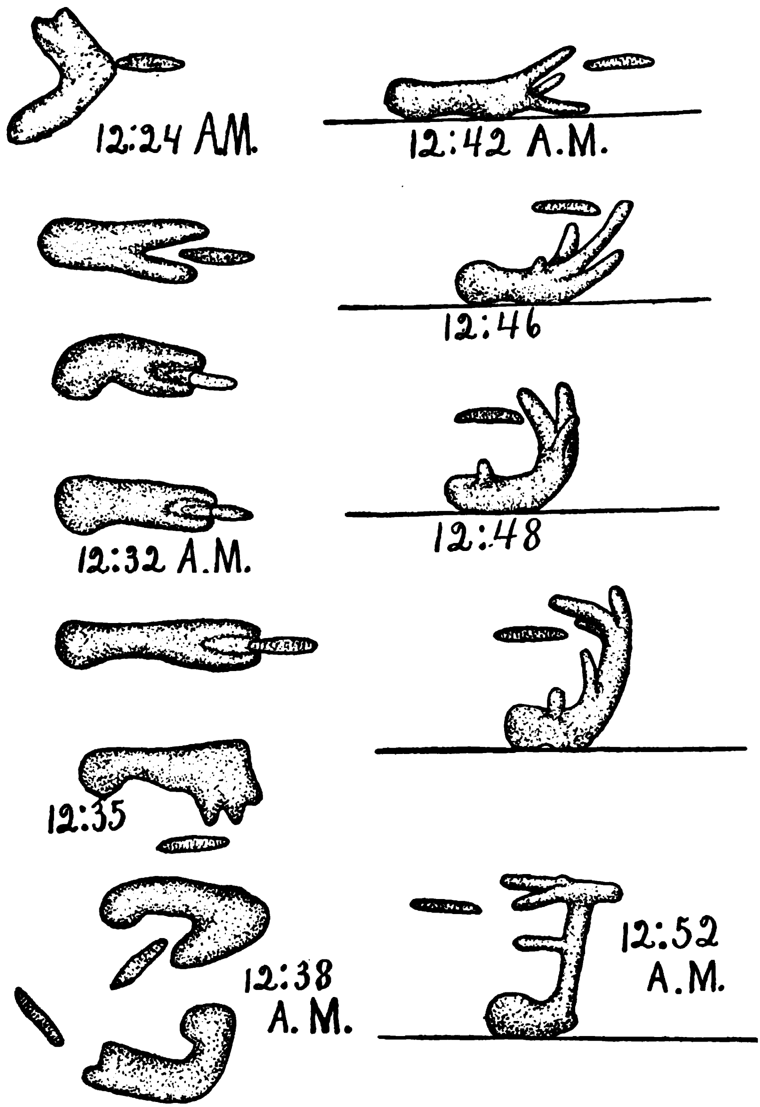


Fig. 7. The "Pursuit".

of the chamber, the escape of the paramœcium, the further "pursuit" at a higher level, and the final escape.

When in "pursuit" in this way the amœba does not generally respond to other stimuli, especially if close upon its prey.

It seems able to detect a paramœcium at some distance, and will continue the "pursuit" some time after the paramœcium has gone from the field. The intensely interesting sight of an amœba after numerous trials gradually sliding its pseudopods around a feeding paramœcium, throwing a cover over it, closing the pseudopods, and gradually squeezing the struggling victim down to a rounded mass can hardly be described without using anthropomorphic terms. It requires a number of adaptations and considerable skill, which our observations seem to indicate are acquired by the "method of trial and error." The capture is seen to be difficult when we compare the reaction times of the two animals. That of the paramœcium is too short to be measured without special chronometric apparatus, while that of the amœba is about one and one-fourth seconds.

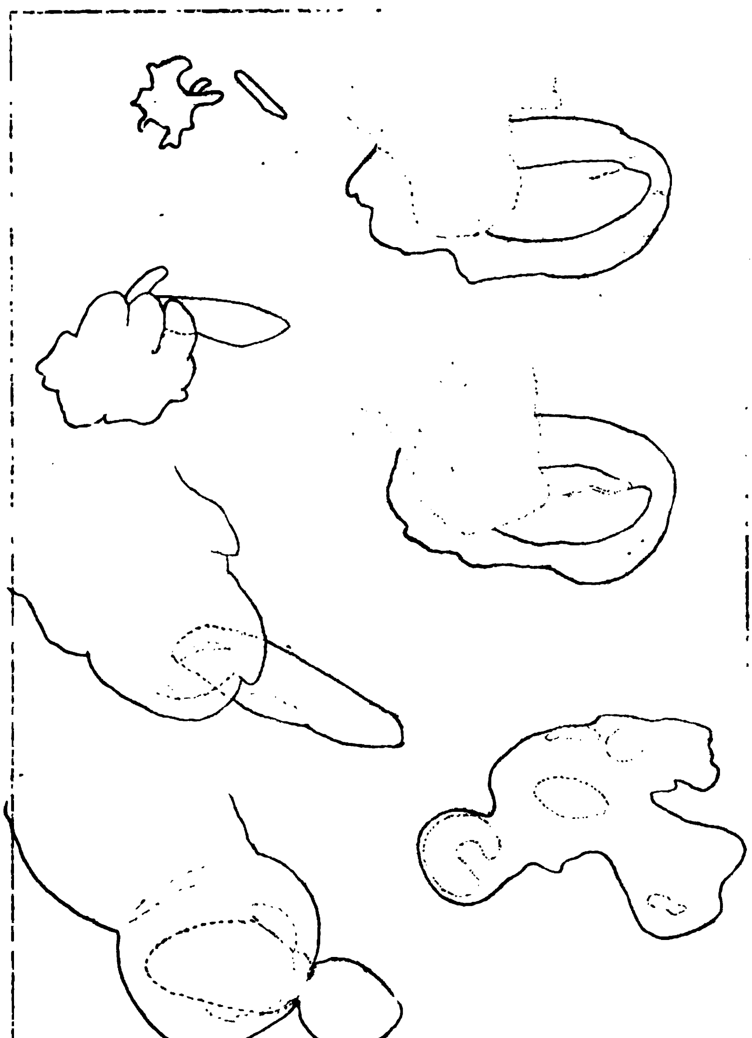
The stages and adaptations of "pursuit" are well shown in the photographs by Dr. Dellinger, Plate III. They show the first stage of "pursuit," the gradual enclosure, the formation of the chamber, the tension or squeezing effect, the enclosure, and finally an amœba containing a recently captured paramœcium and the partially digested remains of four or five others.

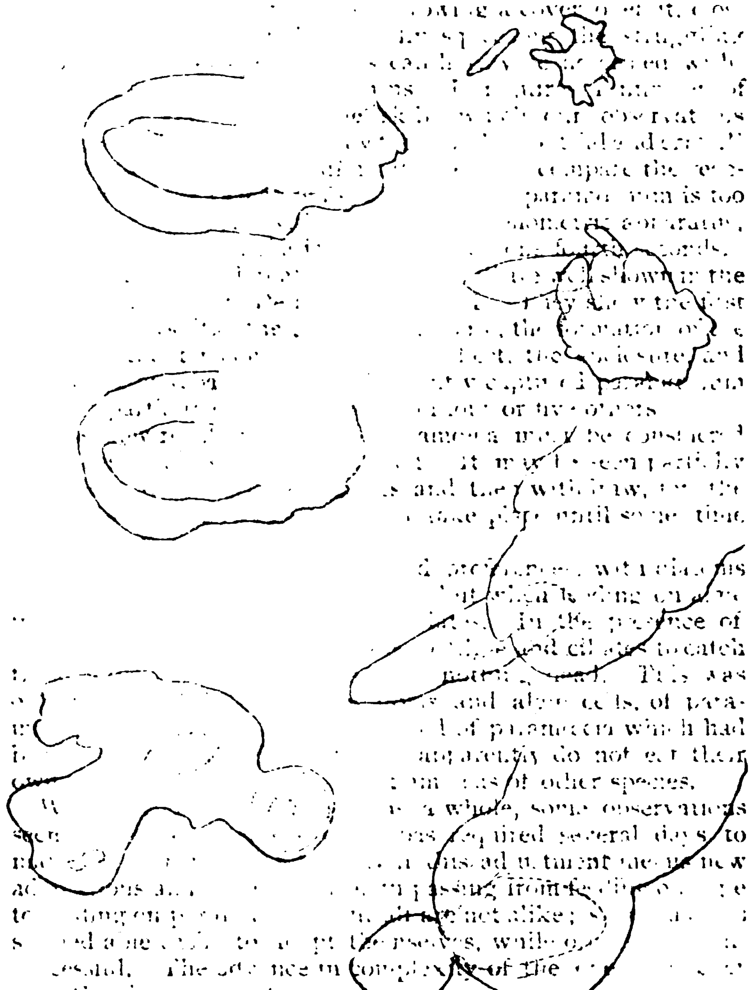
This slow reaction time of the amœba must be considered when describing its choice of food. It may be seen partially to enclose dead cells or ciliates and then withdraw, for the reaction-choice apparently cannot take place until some time after contact.

The amœba shows distinct food preferences: with diatoms and unicellular algæ, it takes algæ, but when feeding on algæ it will leave them to "pursue" ciliates. In the presence of large paramœcia, some amœbas leave algæ and ciliates to catch these larger forms. Amœba eats nothing dead. This was observed in the case of dead diatoms and algæ cells, of paramœcia dead from natural causes, and of paramœcia which had been artificially killed. Amœbas apparently do not eat their own species, but were seen to eat amœbas of other species.

When the food is changed as a whole, some observations seem to indicate that many amœbas required several days to make the adjustment, especially if this adjustment means new adaptations and movements, as in passing from feeding on algæ to feeding on paramœcia. But all are not alike; some amœbas seemed able easily to adapt themselves, while others were unsuccessful. The advance in complexity of the one stage over the other is very great.

An amœba suddenly placed in the midst of a large number of paramœcia, which bump it and knock it about, usually makes no response to the separate stimuli, but seems "confused." Later, some amœbas in these circumstances, put out pseudopods and may "pursue" a single paramœcium without





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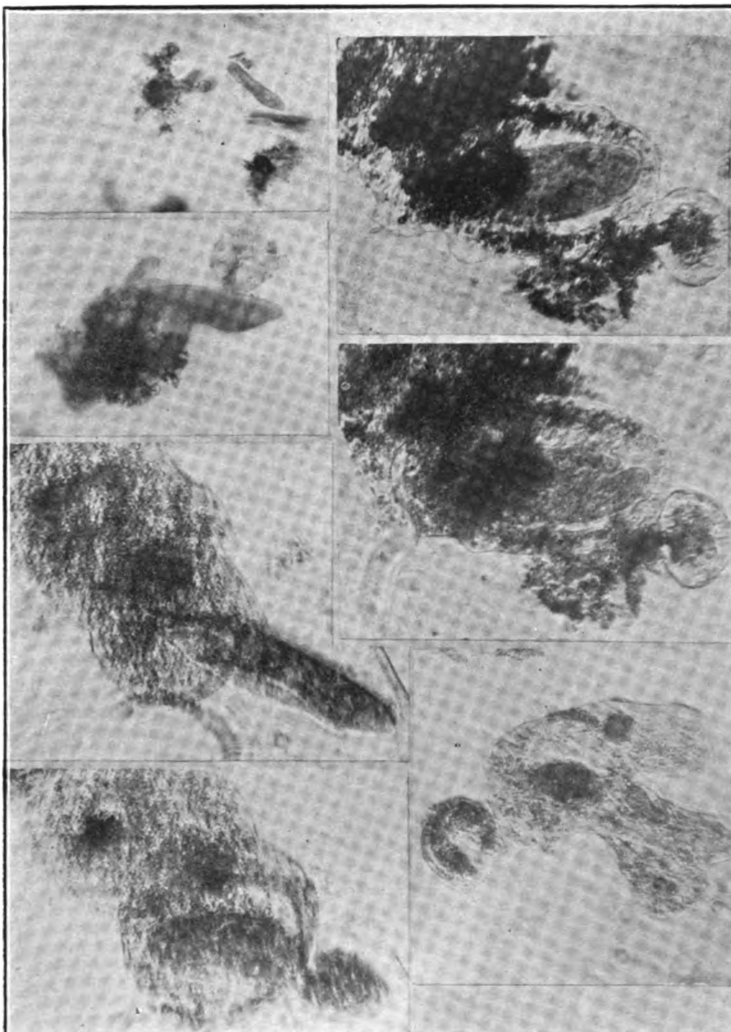

PLATE III.

much regard to touches from the others; while some appear never to get their equilibrium, but move off or take the spherical form.

A number of minor observations should be noted. The action of the contractile vacuole was carefully observed. It was found that the rate of its pulsation varies greatly, but was slowest in nearly rounded and sluggish forms, and more rapid in actively moving forms, that it empties through the ectosarc with a distinct mouth, that some fluid is discharged, that the vacuole usually, if not always, reappears at the same spot. The evacuation of particles was also frequently seen taking place from a food vacuole, the particles being expelled by contraction of the vacuole.

Although amœba were often found in contact, no trace of conjugation was observed. On the other hand, they were frequently observed to avoid each other.

From the facts observed it would seem that

1. The *amœba proteus* in common with higher animals has distinct periods of work and rest, depending for degree and duration upon the nature and abundance of food upon which the animal is habitually feeding.

2. That the *amœba proteus* has food preferences and in general the power of adapting itself to changes in food conditions. This power of adaptation and of choice is perhaps the result of a learning process based upon the "method of trial and error."

3. That the *amœba proteus* is capable of a sort of "pursuit" with the various adaptations which this involves.

The study seems to show that amœba can no longer be considered as a bit of but slightly differentiated protoplasm, but must take its place in the true animal series with the rudiments at least of true animal behavior.

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MINOR COMMUNICATIONS

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

V. The Effect of Imperceptible Lines on the Judgment of Distance

By Helen M. Manro and M. F. Washburn

Experiments by Titchener and Pyle,[1] reported to a recent meeting of the American Philosophical Society showed, in opposition to those of Dunlap,[2] that imperceptible shadows so placed as to supply the end lines of the Müller-Lyer illusion did not affect the apparent length of the lines at the end of which they were situated. Below is stated the result of some experiments by a somewhat different method, bearing on the same point. They were suggested by the experiments on "subconscious" perception described in Sidis's Psychology of Suggestion, 167 ff.

The apparatus consisted of two cards, on each of which a horizontal line was drawn in ink. The lines were both 15 cm. long. At the ends of the one, two faint lines, 6 cm. long, extending outward at an angle, were drawn in lead-pencil; at the ends of the other, similar lines extending inward. That is, these lines were in the position of the end-lines in the Müller-Lyer illusion. The cards were then held together, the one above and a little to one side of the other, at such a distance from the observer that the pencil lines were just not visible. The observer was asked to judge which of the lines was the longer. According to Sidis, a letter or figure too far away to be read may be correctly guessed with the aid of subconscious powers of discrimination, and we wished to see whether in a large number of experiments under these conditions the line with the imperceptible out-going lines would be judged longer a greater number of times than the other. The results may be stated in a sentence. The tests were made on ten fairly practiced observers, no fewer than a hundred on each observer. The total number of experiments was 1,370, and in 700 of these the judgment was in accord with the Müller-Lyer illusion. In the case of only two of the observers did the proportion of judgments showing the illusion rise above one-half. In one of these cases, the total number of experiments was 210, and the number showing the illusion was 136. In the other, the total number was 200, and the number showing the illusion was 132. It seems improbable that the lines at the ends had any influence upon the judgments, except possibly in these two cases. Our results are thus in accord with those of Titchener and Pyle.

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[1] This *Journal*, xviii, 388; *Proc. Amer. Philos. Soc.*, xlvi, 1907, 94.
[2] *Psych. Rev.*, vii, 435.

VI. The Motor Memory of the Left Hand

By Lucy Rowe and M. F. Washburn

The object of these experiments was to compare the ability to reproduce 'nonsense figures', by means of the left hand, with that shown by the right hand in similar tests. The method used was that employed by Binet to test motor memory, which may be found described on p. 397 of Titchener's Instructor's Manual, Qualitative. The observer's hand was guided by the experimenter along a nonsense figure of six lines, the eyes being closed. The figure was then reproduced from memory. In the course of the whole series of experiments, the same figures were used twice, once in testing the right hand and once in testing the left hand, but with a sufficient interval between, during which other figures were used, to prevent memory of the preceding test. The results were evaluated as follows. A line which was reproduced in the direction which it had in the copy was counted correct. Lines that were reversed, that is, directed to the left when they should have been to the right, were recorded under a separate heading, as were lines that were upside down. When the general direction, right, left, up or down, of a line was correct, but the angle made with the preceding line was too large or too small, the record was made under the head of "Error in angle". The results appear in the following table:

	Right Hand.				Left Hand.			
Obs.	Correct.	Error in angle.	Rev.	U. d.	Correct.	Error in angle.	Rev.	U. d.
WO.	535	119	27	15	551	145	30	10
S.	322	14	21	6	349	15	47	4
H.	282	94	27	28	237	76	54	19
ST.	480	51	43		487		47	45
R.	173	14	36	8	181	19	17	12
L.	254	48	25	16	271	36	36	14
H.	414	106	31	10	465	77	41	19
P.	239	43	46	15	243	25	57	20

We had rather expected that the right hand would show superiority over the left hand in these tests. It will be seen from the table that in every case but that of observer H. the left hand gave a larger number of correctly reproduced lines. The introspection of our observers suggested a plausible explanation for this fact. We are so unaccustomed to performing accurate movements with the left hand that attention to its experiences in the giving of the copy is more strained and apparently more effective. The movements of the right hand, on the contrary, are more nearly automatic, and being less attended to are less accurately reproduced.

It might be expected also that the left hand would show a stronger tendency to reverse movements in reproducing them than the right hand. The figures show that this was the case with every one except observer R., though the difference between the hands was not very marked.

VII. A Study in Guessing

By Marie Stroh, A. Margaret Shaw, and M. F. Washburn

The experiments to be described were suggested by those of Sidis,

described on pp. 168-171 of "The Psychology of Suggestion." Cards bearing printed letters or numbers were in Sidis's tests held at such a distance from the observer that they could not be read, and the observer was asked to guess the letter or number on the card shown. Sidis found that in a certain percentage of cases too large to be due to chance the guesses were correct, and argues that a secondary self, endowed with better powers of vision than the primary self, influenced the guessing.

Our own experiments fall into three series. In the first of these, a procedure like that of Sidis was followed. The cards used bore each of them one of the first ten letters of the alphabet, and they were held at such a distance that the observers could barely detect the letter as a faint spot on the card. The observer was told that the letter on the card was one of the letters from A to J. In a large number of experiments, then, the probability would be that one-tenth of the guesses at the letters would be correct, if the guessing was not subject to any influence.

In the second series, the conditions were rendered more difficult by enclosing the letters in rectangles. It was thus made almost impossible to be guided in guessing by the general bulk of a letter,—as, for instance, B might in the first series be distinguished from I.

In the third series, the letters were whispered instead of being shown on cards. It was found necessary in this series to rule out every experiment where the observer heard the slightest sound from the whispering. If anything at all were heard, it often caused the letter to be recognized, especially such letters as C, G, H and J. The experimenter would therefore give the observer a 'Ready' signal, and then whisper the letter so softly that no sound whatever could be heard at the distance at which the observer sat.

The following tables show the results of these series:

Series I. Printed letters without enclosing rectangles.

Observer.	No. of Experiments.	Per cent. of correct guesses.
Str.	100	38
S.	100	32
M.	100	38
R.	200	33
Si.	126	48
W.	310	24
H.	200	51
B.	50	16
L.	50	22
P.	50	16
Bo.	100	55
E.	100	74
Li.	330	46

Series II. Printed letters with enclosing rectangles.

Observer.	No. of Experiments.	Per cent. of correct guesses.
Str.	100	37
S.	100	21
Si.	100	24
W.	240	63
P.	50	18
L.	50	34
B.	50	8
E.	200	67

Series III. Whispered letters.

Observer.	No. of Experiments.	Per cent. of correct guesses.
Str.	600	31
S.	400	19
M.	100	12
H.	400	16
L.	250	10
W.	200	39
Bo.	100	13
E.	106	16
Si.	200	24
P.	200	23

From these tables it appears that only one observer, B. in Series II, ever fell below 10 per cent. of correct guesses, the proportion required by mere chance, and that in this case the number of experiments was so small that the law of probability would hardly apply. In Series I, some of the observers, for instance E.,Bo.,H., and Si.,obtained so high a percentage of right guesses as to suggest that they must have been almost able to read the letters, although they declared in good faith that they could not. The possibility of reading in the ordinary sense was much less in the second series, yet two of the observers, E. and W., guessed right in more than half of the cases. In Series III, where the letters were whispered, since every case in which the observer heard the slightest sound of the whisper was ruled out, the conditions should have made ordinary perception impossible. It is noteworthy that although no observer fell below 10 per cent. of right guesses in this series, L., M., and Bo. had but little above that amount. Yet Str. and W., with 31 and 39 per cent. respectively, show that their guessing must have been somehow influenced quite decidedly in the right direction, and the others also give evidence of such influence, though in a less marked degree.

Our results, then, confirm, on the whole, those of Sidis and show that with certain observers at least judgments may be influenced in the direction of correctness when the observer is unconscious that any such influence is present. Whether this effect is due to a secondary self with superior senses, as Sidis believes, or to a physiological result of the stimulus, too slight to affect consciousness on its own account, as it were, is a question to which our experiments can furnish no answer.

VIII. A Study of Errors in the Perception of Movement on the Skin

By Ruth Hoag, Julia A. Lindemann, and M. F. Washburn

The object of this study was to test the generally accepted statement that movement and rest can be correctly discriminated when the direction of the movement is not accurately perceived, a fact which Külpe explains by the law that "general or abstract names are more easily reproduced than concrete." Movements of very slight extent on the part of a tactual stimulus were employed. The observers sat with the left arm extended on a table, and with their eyes closed. An ink-dot was made on the volar side of the wrist, about 5 cm. above the hand. Four other dots were placed at distances of one mm. from the first, one each in the central, peripheral, radial, and ulnar direc-

tions. The rubber point of an ordinary æsthesiometer was placed upon the skin at the centre dot, and either moved in one of the four directions to another dot, or held upon the centre dot for a period as nearly as possible equal to that occupied in a movement, that is, about one second. The observer judged whether the point had remained at rest, or had moved, and the direction of its motion if it was thought to have moved.

Twelve observers, all women, served in the experiments, and 7,100 experiments, in all, were made, no fewer than 500 on any one observer. It seemed at the conclusion of the study that the distribution of the errors made in so large a number of experiments was not without interest. A table showing it is therefore presented.

Actual Movement of Stimulus	Judgments: Central	Peripheral	Ulnar	Radial	Rest	Total Errors
Central	1094	45	58	106	117	326
Peripheral	51	1050	80	83	156	370
Ulnar	123	90	1020	43	144	400
Radial	119	84	15	1083	119	337
Rest	63	52	54	46	1205	215
Total Errors	356	271	207	278	536	

The results that appear from this table may be summarized as follows:

1. A resting stimulus is judged correctly as resting oftener than any direction of movement is correctly perceived.

2. On the other hand, when a moving stimulus is incorrectly perceived, it is more likely to be judged as at rest than as moving in a direction other than its real direction.

3. Next to a resting stimulus, the order of accuracy in the perception of the stimuli is: central, radial, peripheral, ulnar. But the superiority of the central direction is crossed by result 4 in the same way that result 1 is by result 2.

4. When the direction of a moving stimulus is incorrectly perceived, it is more likely to be called 'central' than any other direction. This is in entire accord with the results of Hall and Donaldson, who say, "We are more likely when in doubt to judge motion on the surface of the limbs to be up rather than down the axis."[1] They suggest that this is due to the fact that movements up the skin would ordinarily be produced only by some living thing, while movements downward would be common experiences through gravitation.

The possibility then suggests itself that instead of rest's being better perceived than the direction of motion, and movement in the central direction better than movement in any other direction, there may be simply a general tendency, when in doubt, to say 'Resting' or 'Moving centrally.' This would account alike for the facts that there were more correct judgments under these two heads than under the others, and that more wrongly perceived stimuli were assigned to these two classes.

Examination of the individual records of the different observers throws some light upon this point. The following facts appear from such an examination:

(*a*) Only six of the twelve observers judged the resting stimulus best of all.

(*b*) Of these, five were more apt to call a misjudged stimulus 'resting' than anything else. Two other observers showed the same tendency.

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[1] *Mind*, O. S., X, 559.

(*c*) Five observers only judged the central direction either best of all or next best after the resting stimulus.

(*d*) In four out of these five cases, the number of wrong judgments that involved calling the direction of movement 'central' was either greatest of all, or second only to the number where the stimulus was called 'at rest.'

It looks, then, as if there were a tendency on the part of some of our subjects to make the judgments 'resting' or 'centrally moved' when uncertain, which may account for the apparent superiority of judgments under these two categories.

IX. A Suggestion towards a Study of the Perception of Sound Movement

By Joyce Hicks and M. F. Washburn

The method used in the experiments to be described was as follows. The observer sat with eyes closed. The experimenter stood either behind her, to her right, to her left, or in front of her, and held a König tuning fork, mounted on its sounding box, at a distance of about 50 dm. from the observer's head and on a level with her ears. Chalk marks were made on the floor 30 dm. to the right and left of the point directly over which the fork was held. The fork used was a C^3 of 1,024 vibrations. The fork was struck with a felt hammer by E and moved 30 dm. to the right, left, up, or down. The extent of the movement to the right or left was guided by the marks on the floor; the movements up or down had to have their extent governed merely by E's attempt to make it as nearly as possible equal to that of the others. The duration of the movements was governed by a rhythmic count 'one-two' mentally made by E. An equal number of experiments was made where the movement was in each of the four directions, and also when the fork was held perfectly still and allowed to sound the same length of time as that occupied by a movement. The same number of experiments, in the four directions and with the fork at rest, was made at four different positions, in front of the observer, to her right, to her left, and behind her. Seven persons served as observers, and 164 experiments were made in each of the four positions, making 656 in all.

Tables showing the results are subjoined.

Position: Front

Direction of Movement	Judgment: Up	Down	Right	Left	Rest
Up	39	27	30	31	37
Down	44	23	33	24	38
Right	25	9	101	13	16
Left	36	23	10	79	16
Rest	29	19	22	5	89

Total correct judgments in this position: 331.

Position: Back

Direction of Movement	Up	Down	Right	Left	Rest
Up	89	30	14	4	27
Down	27	65	22	15	34
Right	21	8	115	8	12
Left	23	11	5	96	18
Rest	28	19	5	10	102

Total correct judgments in this position: 467.

Position: Left

Direction of Movement	Judgment: Up	Down	Right	Left	Rest
Up	68	8	22	38	28
Down	25	49	23	50	15
Right	28	17	60	24	35
Left	32	10	34	44	34
Rest	16	22	14	14	98

Total correct judgments in this position: 319.

Position: Right

Direction of Movement	Up	Down	Right	Left	Rest
Up	72	14	38	17	23
Down	36	26	45	28	28
Right	41	27	55	21	21
Left	42	20	17	58	27
Rest	23	10	14	12	104

Total correct judgments in this position: 315.

It is unnecessary to point out how rough the method employed in these tests was. There was no accurate control either of the extent of the movement, or of its rate, or of the intensity of the tone. Yet all three of these defects represent variable errors, and in so large a number of experiments they would approach elimination. In any case the study may be useful in suggesting a problem to some one who has more accurate apparatus at his command. A few of its results are sufficiently decided, both in the tables given above and in the figures from the individual observers, to claim some validity. They are as follows:

1. The direction of sound movements is better perceived when the moving tone comes from behind the observer than when it is in any other position. There were 467 correct judgments in this position, whereas 331 was the highest number in any other position. This is interesting because of the tendency noted in experiments on the localization of a resting sound to throw the sound back, a tendency which Gamble explains as due to "serviceable reflex movements in response to noise".

2. When the tone was at rest, the fact was more accurately perceived than was movement in any direction. The total number of correct judgments of the resting tone was 393; the next highest number was 331 for movement to the right. This superiority of the judgments of rest does not seem to be counterbalanced here, as in the experiments on tactile perception of movement, by any tendency, either general or on the part of individuals, to say "no movement" whenever in doubt.

3. Movement downward was on the whole most poorly perceived, the number of correct judgments being 163, and the next higher 268 for movement up.

This may be due to the same cause that renders sounds less easily localizable in the median plane.

THE IMAGINATION OF ADOLESCENTS

By Walter Libby

Under this title I wish to record a particular investigation carried on during the last two years. No attempt is made to treat the subject of adolescent imagination comprehensively or even to show all the pedagogical inferences to be drawn from this special study. In fact the scope of what I have to say is still further limited, as the investigation concerned itself merely with one aspect of the general topic, namely, the relation of the imagination of school children to their feelings.

In the first place there was presented to a fourth year class of Illinois High School pupils a somewhat sentimental picture on which they were asked to write a composition. In the foreground of the picture a young lady stands at an old-fashioned gate weeping, and in the background a horseman in white suit and cocked hat is riding away. The class to whom this was first presented consisted of forty-one,—seventeen boys and twenty-four girls. The compositions written by them showed that the picture appealed strongly to their stage of development. All the students wrote fully and with apparent spontaneity. The teacher, who wrote a few comments on the margins, was inclined to prune away epithets and to check the exuberance of the style. The "fine writing" of which the pupils were accused by their preceptress seemed to me to spring from the exaggerated ardor of their feelings as gauged by maturer standards. If these so-called faults in writing are merely matters of literary style, then style must be considered very vital and personal, and to really modify it would involve a reformation of the pupil's nature.

In order to establish a basis of comparison this same picture was presented to pupils in an Illinois grade school, namely, to twenty-three of fourteen years of age and over, to fifty-one of thirteen years, to thirty of twelve years, twenty-three of eleven and seventeen of ten. Needless to say there was a very marked difference in the compositions from the grades and those from the fourth year High School class. This difference can be briefly described by saying that the compositions from the grades were largely objective, while those from the High School class were decidedly subjective. Also noteworthy is the fact that the line of cleavage occurs after the fourteenth year. I was led to make a somewhat closer study of the returns from the twenty-three grade pupils of fourteen years and over as compared with the returns from the fourth year High School pupils. I found that the general and rather vague distinction indicated by the terms subjective and objective admits of more definite statement. Of the twenty-three grade pupils twenty,—nine out of nine girls and eleven out of fourteen boys,—mentioned "the picture" as such, while not a single such reference occurs in the forty-one High School productions. All the latter entered at once sympathetically into the situation portrayed. The picture aroused their emotions and *ipso facto* stimulated the imagination.

Closely related to the objectivity of the work in the grades is the inclination to give a number of loosely connected details. In the High School compositions the observations are unified by the underlying emotion. This might be stated from the point of view of the teacher

of rhetoric. The more mature productions are marked by greater unity, more careful explicit reference, and a more complex sentence structure. To deny that the language teacher is to be credited with a considerable share in this improvement would be to take a cynical attitude towards the profession, but to deny on the other hand that the natural evolution of the adolescent powers is a large factor in this change would be to ascribe to language teaching a more artificial character than it deserves. The teacher of composition can exhort the pupils to select in writing a certain point of view, but in imaginative writing the magnet that draws to a centre the details of the work of art is the emotion of the writer. To illustrate from our returns, in eight of the forty-one High School compositions the mood, the emotional tone of the essays, was struck by the words "Never to return," but no such phraseology is found in the work from the grades. It may be worth noting that seven out of the eight compositions in which this melancholy refrain occurs were written by adolescent girls. Sixteen of the High School boys took it for granted that the man on the black charger was the young lady's lover riding away; one took him for a young husband. Of the twenty-four High School girls eighteen thought he was a lover, one a messenger, one a father, and four a brother. Of the fourteen grade boys five took the horseman for a husband, one for a lover, one a son, one a son or brother, the remaining six failing to see him or to specify. Of the nine grade girls four thought him a husband and one a lover, the other four neglected the question.

Fully one-third of the older students imagined the hero and heroine to have been friends in childhood, while to only one of the grade pupils did this idea occur. In this matter we might think that the adolescents had been influenced by their reading of novels, especially as childhood friendship is mentioned by the boys almost as frequently as by the girls. Certainly imitation of the fiction read by these students would strike the most careless reader of these essays, but we must expect that the adolescent mind adopts for its own that which really appeals to its nature.

It is, of course, obvious that their studies and more serious reading must influence and give balance to the pupils' imaginative constructions. Of the High School boys ten make Germany the scene of the plot and one, France. Of the High School girls eight locate the incident in Germany, five in France, four in America, two in Austria, and one in England. Of the grade boys one mentions Germany, two America, besides one who calls the cavalier a cow-boy. The grade girls disregard the geographical question. A large percentage of the High School pupils mention definite times like the war of 1870, of 1696, and the war of the Austrian Succession. Among the grade pupils two mention the War of the Revolution, one the Colonial Period, one "once upon a time" and one "olden times." That the incident portrayed in the picture occurs in war time is taken for granted more frequently in the High School than in the grades, and more frequently by the boys than by the girls.

Although, as already stated, the compositions in the grades are markedly detailed, a single detail, the autumn leaves lying on the ground, is more frequently mentioned by the older pupils. The High School girls especially make a point of the dead leaves. By them, however, the leaves are not considered merely as a detached detail, but are given unity, by their emotional suggestiveness, with the whole story. They are symbolic of the melancholy of a touching farewell. The High School pupils, and again especially the girls, are bolder in the use of ornate and unusual epithets. The grade pupils seldom venture

beyond such descriptive terms as "swampy" and "fine," although in one case "sad-looking" was used. The epithets used by the High School girls indicate that they are more eye-minded and more ear-minded, less motor-minded than the boys. One feels in reading these compositions how great a part is played in emotional scenes by faintly heard sounds, impressions of temperature, and of slight pressures, such, for example, as are produced by a passing breeze. Among the adolescents again greater imaginative sympathy is indicated by the use of direct discourse, the first person and proper names. I was disappointed by the absence of burlesque in the High School compositions. Only one boy gave the comic muse rein, suggesting that the moral of the story was "that soldiers, book-agents and travelling men generally were not to be trusted."

One of the most characteristic things about the High School returns is the rhythm, the musical swing of the sentences and paragraphs. The imagination seems under the sway of a musical mood. The likeness of the compositions to popular fiction is in this respect remarkable. One might describe this rhythm as a sentimental languorousness, similar to the cadence of a Strauss waltz. This characteristic could, I think, be expressed less vaguely in the terms of rhetoric.

I shall not go into details in reference to the latter part of my investigation. After establishing the distinction between the imaginative work of students of eighteen years of age and those of fourteen, I submitted this same picture to the four classes of an Illinois academy in order to show stages in the development of the imagination during the High School age. Later a picture of a Roman Chariot Race was presented to the four forms of an Ontario Collegiate Institute. It appeared from both of these sets of returns that the great emotional and imaginative change in adolescents came after the age of sixteen. The chariot race met with less comprehension and appreciation from boys of fourteen and fifteen than I had expected. Occasionally all enthusiasm for the race was smothered by a schoolmasterly, expository spirit.

A popular picture with an ambiguous title, *In Full Cry*, was given as a subject of composition in the four forms of a second Ontario Collegiate Institute, and also in the four forms of an Ontario Normal School, where the students average twenty-one years, in order to secure a basis of comparison between High School students and those of maturer age. Finally, a picture called *A Serious Affair* was presented to the four forms of a third Ontario Collegiate Institute and to three forms of an Ontario High School.

The whole investigation confirms the views of various experts as to the close interrelation of the imagination and the feelings. What differentiates prosaic thinking from imagination is the presence of the feeling factor. All the emotions serve as stimuli to the imagination. A genetic view of the emotions has shown in recent years their importance in the maintenance of individual and racial life. If the connection of the imagination with the feelings be firmly established, the vital value of the functioning of the imagination can be inferred. It is no semi-superfluous power that may be exercised or held in abeyance at will, but an aspect of consciousness that may be discerned in every mental process of capital import. I am led, therefore, to dissent in part from the conclusion reached in certain recent investigations similar to mine, which, unduly emphasizing the study of the image, and neglecting the functional aspect of the imagination, claim that the imagination is poorer in the pupils in the High School than in those of the grades. Adolescence is the birth of a richer emotional life, which is dependent in turn upon a wider range of associations,

and upon physiological changes, including the rapid growth of the heart and the development of the tangential fibres in the brain. At the same time adolescence is the dawn of the brightest and most vivid imaginative period. To claim that poor High School teaching has actually reversed the order of nature seems to me to claim too much for defective pedagogy. That our High School teaching is wanting as judged from the standpoint of the psychology of the imagination I feel forced to admit. Much of the work is trivial, desiccated and barren. The imagination shown in our secondary schools is meagre in comparison with what it might be. But High School students have not lost the imaginative function. It would be truer to say that we instructors have lost the control of this vital activity and that cheap fiction is usurping the domain of the educator. When we consider the importance of the imagination for the moral life, that in our imagination we rehearse our virtues and our vices, we must envy the novelists, actors, elocutionists, and artists who sway the imagination and its underlying emotions.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

XXII. Children's Capacity for Abstract Thought as Shown by Their Use of Language in the Definition of Abstract Terms

By Louise Ellison

The close dependence of thought in all its higher forms upon language has been universally recognized. To think in any but the most concrete way means almost without exception to think in words. Without abstract terms generalization can reach but a low level; and at its higher levels the word (or symbol standing for it) is the sole means of holding and using the general idea.[1]

But this is not all; in not a few cases the learning of the abstract term is an important step towards becoming acquainted with the abstract idea for which it stands. The word assists in sharpening attention and gives notice that an idea corresponding to it exists and may be found. How much of human capacity in dealing with abstractions depends on the fact that children are born into a speaking environment would be hard to estimate, but the influence is certainly not small.

Facility in the use of language is in some degree, therefore, an index, though of course not the only one, of mental development. The age at which children learn to speak and the number of words which they can understand and use at various ages have long been a matter of interest both to parents and to students of the early stages of child life, and several laboratory tests turning exclusively on knowledge of words or skill in handling them have been proposed for determining mental condition at different ages.

A step beyond the ordinary comprehension and correct use of abstract terms is required, however, for the exact definition of them. No one has reached the highest level of skill in abstract thought until he is able to state the precise scope and limitations of the terms that he uses. The method and the success of attempted definitions may thus serve as still another measure of attainment in the power in question.[2]

In view of all this, a study of the ability of children of different ages in the defining of abstract terms might be expected to give useful information with reference to their advancement in the power of abstract thought and no doubt would do so, if carried out under favorable circumstances and with due regard to the correlation of

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[1] Ribot: Evolution of General Ideas.

[2] Definition, from a psychological point of view, is an effort to call up in the mind of another an adequate conception of an idea in the mind of the speaker. Many means may be employed for thus "conveying the idea." The child or the practical man often does so by giving a sample—by citing an instance or giving an example or even by pointing out, or showing, what is meant. The logician defines by genus and difference. Between these two extremes there is a considerable range of procedure, some forms of which appear in the data to be considered.

power of this sort with that in other directions. The present study goes but a short step in that direction, however, for the material, while collected under the supervision of a university professor in the schools of a western city and kindly placed at the disposal of the writer by him, is adequate only for a study of the relation of age and sex to defining ability in a mixed group of children, and furnishes no information upon a number of points upon which information would be desirable. The relations which seem to be clearly established can be stated briefly.

The material consisted of the definitions for twenty-seven more or less abstract words in common use written by boys and girls of from eight to fifteen years of age. These were gathered by the teachers under the guidance of the following circular of instructions.

"To the Teachers:

(1) Please ask the children in your room to write on paper the meaning of each of these words.

(2) Have each child write his or her *first* name and age at the top of each sheet of paper used.

(3) Write the words on the blackboard, but only so many each day as may surely be defined during that period.

(4) Pronounce a word as often as a child wishes, and in the lower grades each word should be spoken as well as written.

(5) It is of the greatest importance that you should *not* give them any information that would suggest a definition, as this would make their definitions useless. Do not tell them not to use in their definitions the word that they are defining. It is the *child's* definition that is wanted.

(6) After the children have written the meaning of all the words, please tie their papers in a bundle, write the grade on the outside and hand the package to your principal.

Words to be defined:

1. hunger	15. thankfulness
2. weight	16. truth
3. slowness	17. life
4. shape	18. hardness
5. color	19. health
6. laziness	20. anger
7. courage	21. neatness
8. strength	22. pleasure
9. goodness	23. man
10. love	24. play
11. length	25. house
12. happiness	26. boy
13. size	27. home
14. use	

The total number of children who answered was 472, of whom 253 were girls and 219 boys. The number of papers for each age and sex was as follows: Girls, 8-year-olds 9, 9-year-olds 44, 10-year-olds 52, 11-year-olds 51, 12-year-olds 47, 13-year-olds 28, 14-year-olds 13, 15-year-olds 9. Boys, 8-year-olds 11, 9-year-olds 39, 10-year-olds 46, 11-year-olds 35, 12-year-olds 40, 13-year-olds 27, 14-year-olds 17, 15-year-olds 4. The total number of definitions considered was about 12,000, some of the children having failed to define some of the words.

All of the definitions which were legible were used in the classification. The basis of classification was for most of the words chiefly the way in which the thought was expressed (*i. e.*, the grammatical or rhetorical form of the definition), but as a rule form and content

ran parallel and content was so far regarded in all as to lead to the placing in a class by themselves of all those definitions that seemed to the classifiers as reasonably adequate. In the case of four words, however, *man*, *boy*, *house*, and *home*, the definitions were grouped according to content chiefly, but even here content and form ran closely enough together to allow these words to be taken into account in the general comparison. In the few cases where more than one definition was given for a single word the first one only was counted.

The classification of the definitions proved to be anything but an easy task; but was, nevertheless, carried through with care. To insure as great exactness as possible the writer first worked over the entire material alone and then a second time in conjunction with Dr. Sanford. The classes finally made when form of expression was the chief criterion are given below. Those made for the words classified according to content will be considered by themselves later.

(1) Definitions by use in a phrase or sentence, as: "Size, My size is very much."

(2) Definitions consisting of the word to be defined followed by "of something," "of anything," as: "Weight is the weight of something." It is difficult to tell whether these really belong to the preceding group or to an independent and early stage of real abstraction.

(3) Definitions involving the mention of a person or thing, as: "Pleasure, somebody that has fun;" "Thankfulness, somebody that is thankful."

(4) Definitions by examples, as: "Color means like red or blue."

(5) Definitions by the use of an adjective, as: "Anger means mad."

(6) Definitions by means of clauses introduced by *when*, *that* or *if*, and having somewhat the nature of examples; as: "Weight means when you are heavy;" "Size means when you are big; when you are fat and chunky;" "Neatness when you are clean." These will be referred to as the when-that-and-if clauses.

(7) Definitions by infinitives, as: "Hunger, to be hungry;" "Thankfulness, to thank somebody."

(8) Definitions by means of synonyms, as: "Happiness means joy;" "Life means existence."

(9) Definitions which, allowing for the child's informal use of language, seemed to the classifiers as on the whole adequate—definitions where the knowledge of the meaning is certain and the thought fairly well expressed. This class will be referred to as that of "Fair definitions." Examples: "Laziness is a habit when you are not ambitious;" "Pleasure the act of enjoying;" "Boy, a young man."

(10) Definitions which resisted classification either because unintelligible or because they could not be brought into any one of the other groups. This class will be known as the "Miscellaneous group." Not all were necessarily poor as definitions; a few might almost have been put in the ninth class, but because of some special peculiarity seemed better classed here.

Besides these ten classes, found more or less frequently for nearly all the words, there were for many words small groups of definitions of forms found rarely or not at all in the case of other words. These will not be further considered.

Though the ten chief classes are moderately distinct as outlined above, there arose in the actual classification a good many cases of doubt and a few in which the decision was felt to be more or less arbitrary. For this reason no importance can be attached to the particular figures found for each class, though the broader relations may still be assumed to hold good, to these alone reference will be made in what follows.

All the words, except *life*, which is *sui generis*, themselves fall into groups within which the proportions of definitions of the various classes are about alike, and of which a single word may be taken as typical.[1] It will be noticed also that these groups, quite apart from the children's treatment of them, belong in a measure to different sorts of abstractions. The type words and their groups are as follows:

PLEASURE: play, use, truth, love.
WEIGHT: size, length, strength.
HEALTH: hunger.
ANGER: courage.
HAPPINESS: neatness, hardness.
THANKFULNESS: laziness, goodness, slowness.
COLOR: shape.
BOY: man.
HOUSE: home.
LIFE.

The definitions, excluding the miscellaneous ones, may themselves be arranged in three, more general groups, which for convenience may be spoken of as (A) Definitions by Sample; (B) Definitions by Abstract phrases, and (C) Definitions by equivalents (including under that head the synonyms and "fair definitions").

A. *Definitions by Sample.*

These include the first five of the classes mentioned on page 255, in all of which the child's effort is to convey the thought indirectly by putting the hearer into a position in which he can grasp it for himself.

1. Definitions by Use of the Word in a Phrase or Clause. In these, so far as the child really tries to give the meaning of the word, he tries to do so by showing how it may be used, and in that way to suggest its meaning. In this procedure he is also very likely copying his seniors; for it is often easier to explain a word to a child by showing him how to use it than in any other way, and often satisfies him quite as well as would a genuine explanation. In the early school years also, much of the child's attention is given to getting command of the mother tongue, which would also incline him towards this sort of definition. Finally, it may be possible that for many of the youngest children the test words were taken in chiefly as auditory impressions and suggested their speech associates without much real conception of their meaning. In any case it is not surprising to find this sort of defining strong with the 8-9-year-olds, and falling rapidly away for the others.

2. Definitions Consisting of the Word to be Defined followed by "of something" or "of anything."

3. Definitions Involving the mention of a Person or Thing.

[It is difficult to tell whether the definitions of these two groups really belong with the illustrative phrases of Class 1, or stand for a stage in which the abstract quality can in a measure be focussed in attention, but cannot yet be wholly separated from the concrete context,—a sort of practical *universalia in re*. In any case the definition operates by reference to a relatively concrete instance. The numbers

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[1] With regard to the differences within these groups the following may be worth mentioning: *Truth* has no examples and no fair definitions." *Play* and *love* have no adjective definitions and few synonyms. *Use* and *courage* are in general poorly defined. All the words ending in "ness" are treated by the children in much the same way, but *thankfulness* and the words grouped with it are defined somewhat more frequently in a way to bring out their active aspect. Other variations of the typical words among themselves, as far as they seem of interest, will be noticed later.

in both cases are too small to allow any certain inference as to the correlation with age].

4. Definitions by Examples. This is the typical case of primitive definition uninfluenced by the linguistic tendency of the first three classes.[1] Where the numbers are of some size, as in the case of the boys, there is a marked decline in this sort of definition as age increases.

5. Definitions by the Use of Adjectives. As evidence of power of abstract thought definitions of this sort stand upon about the same level as those of the second, third, and fourth classes. The correct use of an adjective involves a recognition of the existence of the quality indicated though not separated from the substance in which it inheres. Definition by the use of adjectives may thus be a verbal means of pointing out the essential quality in the thing defined. The precise words used will vary of course with the child's power of expression and his desire to avoid using words of the same root as those defined. He may say, "Laziness means lazy," or "Laziness means tired," according to his vocabulary and his notion of what is required in defining, without essentially differing in his mastery of the abstract idea. Definitions of this class like those by use of the word in a phrase or sentence belong on the whole to the younger children and decline noticeably with increasing age.

B. *Definitions by Abstract Phrases*

6. Definitions by When-that-and-if Clauses. This class marks, perhaps, a certain advance upon that of the adjectives, but like the latter shows a characteristic decline with increasing age. The typical forms, such as "Happiness, when you are happy," "Laziness, means that you don't want to come out of the bed," "Love means if you love that girl," all define by citing instances; but citation by a somewhat indefinite phrase indicates, perhaps, a little loosening of the abstract quality from its matrix in the concrete.

7. Definition by the Use of the Infinitive. With the definitions by means of infinitives we seem to pass from the lowest grade of definitions to a middle grade, though even here not a few of the definitions really cite examples. The infinitive is the most abstract form of the verb (except the participle) and usually in the definitions studied carries neither subject nor object. The following may serve as instances in which the infinitive seems to stand for a middle degree of abstraction: "Heaviness, to be heavy;" "Thankfulness, to be thankful;" "Truth, to be true." In the following it seems rather more like an example: "Hardness, to work hard;" "Play, to play games; to play dolls." Taking both sorts of infinitives together the figures show a rise in frequency up to twelve years with a slight (though perhaps accidental) fall for the 13-14-15-year-olds.

C. *Definitions by Equivalents*

8. Synonyms. Definitions by synonyms may seem to testify rather to an increased vocabulary than to an increased ability to handle abstractions, and doubtless it does so, but the choice of synonyms as a means of definition indicates at least a neglect of the more primitive forms and thus a certain advance. The increase of this form with age is noticeable in the case of both boys and girls.

9. Fair Definitions. These are infrequent with any of the children and nearly or quite lacking in those under ten or eleven years of age.

Summing all this up in a single word we may say that such a mas-

[1] Lloyd Morgan: Introduction to Comparative Psychology, 2 ed., p. 229.

tery of common abstract ideas and of language as would make possible reasonably perfect definitions is nearly or quite wanting in children of thirteen and under, though for several years before that time the abstractions have been clearly enough grasped and the vocabulary has been large enough to allow a good deal of definition by synonyms. Still earlier the children seem to hold the idea in a distinctly concrete fashion and to convey it, when required to do so, by means of concrete examples variously expressed, while the youngest children of all in many cases cite examples of the use instead of really giving the meaning of the word, possibly for the reason that at that stage the idea itself cannot be brought clearly before consciousness.

Definitions by Negatives. Definition by negative forms includes such as the following: "Truth means no lie," "Happiness is not sad," "Laziness, somebody that does not want to work." The negatives for each word were compared with the number for every other word with the following general results: *Hunger*, from the very nature of the term had so many negatives (negation being implied even where it was not expressed) that it was classed by itself. Excluding *hunger*, *truth* has more than one-fifth of the total number of negatives for all the words. The form they take is "not to lie," "not false," "no lie." *Laziness* comes next with a little less than one-fifth. Here the negatives are used in such expressions as, "not to work," "don' t want to do anything," and the like. Next comes *health* with expressions like, "not sick," and *slowness*, with such as "not fast," "not to go fast." There are none for *weight*, *length*, *size*, *use*, *house*, and only two or three each for the remaining words. There is a general increase of definitions by opposites or by the use of the negative from the younger to the older children. For *love* and *color* where there are only a few negatives to compare there is an exception, but not a marked one, to this statement.

As regards differences between the boys and the girls in ability to define, it seems certain that the girls succeed better than the boys at almost all points, getting rid earlier of the more primitive methods of indicating the idea and taking up earlier the more advanced.

Words Classed according to Content

The words *man*, *boy*, *house*, and *home* were classified according to content and not especially by form, as has already been said, though a number of the forms common in the other group occurred here also. The definitions for *man* have nothing corresponding to "a child" which constitutes a large class for the word *boy*, but the main classes are the same and are as follows, the classes being mentioned in the order of their size.

(1) *Fair Definitions*. These include logically correct definitions. Sometimes they are not expressed definitely, but where the idea is clearly correct the definitions were given the benefit of the doubt. Examples are: "Boy,—male child;" "before man;" "the first stage of man;" "a half sized man."

(2) "*A human being*" or "a person."

(3) *Definitions based on size*, as "man, a grown up human body;" "a male being about full length." This group decreases with increasing age.

(4) *Definitions making reference to sex*. It is interesting to note the sudden increase of definitions of this sort at about thirteen years, especially with the girls, notwithstanding the fact that the fair definition group increases too. The usual form of this definition is, "a male person," or "a male." A definition given by a twelve-year-old girl

emphasizes this sex distinction as follows: "A child that is not a girl and is born from birth as a boy."

There are also smaller classes of definitions common to both words, for example, definitions according to the clothes worn, examples and sentences. All of these appear for the most part with the younger children.

The definitions of *house* and *home* are largely interchangeable. For both words the largest class is, "where you live," "where people live," and it increases with the age of the children.

A few of the other words also show interesting peculiarities when the content of their definitions is studied. Out of all the verbal expressions for *love* including when-that-and-if clauses and infinitives, nine-tenths are active in form as, "when you love somebody," "to love somebody," and kindred expressions, while only about one-tenth are passive as, "when somebody likes you," "to be loved," etc. This is certainly a remarkable showing, but is, perhaps, not an inexact picture of the relative subjective importance of the experience in the case of the normal human being. Four girls and four boys define it as "when you like each other." Definitions of *love* as specifically between the sexes were relatively infrequent, as might be expected from children of these ages.

For about three-fourths of all the children *truth* is simply "telling the truth," or "not to lie;" for one-third it is "the opposite of falsehood" or "untruthfulness." It will be remembered that the negatives for truth far outnumbered those of most other words, and these belonged to the "not to lie" or "no lie" class.

Laziness, like *love*, is defined from both the active and passive sides, as "to sit or lie around," and "a tired disagreeable feeling." The latter is by no means infrequent.

Most definitions of *life* turn on such expressions as "to be alive" or "to be living," but there are a few more definite expressions as: "when your heart is beating," "when there is breath in the body," "when you have got feeling."

With a view of comparing the definitions of the children with those of a group of adults, seventeen of the words used with the children were given also to the members of one of the psychological classes in Clark University, fourteen in all. In the case of a part of the words they were asked to note introspectively anything of importance to be observed in the framing of the required definitions.

The chief thing shown by these introspections is the great mass of associations which a word calls up. The definitions show a greater command of language and the influence of reading and of book definitions, but on the whole, were not strikingly different from those of the children. Four-fifths of the students stated that the word called up a concrete image.

The children's definitions were also compared with those of the dictionaries. The children and the dictionary makers differ in that the latter give fuller and more exhaustive definitions. Dictionary makers view a word from every aspect, while the child considers only one. This distinction may be due to the child's meagre associations, to his lack of facility in handling language and to his lack of time. Yet for the aspect which the child does define he often expresses the meaning nearly as well as, and sometimes better than, the dictionaries, though his mode of expression is often questionable from the standpoint of rhetoric and grammar.

For nouns derived from the adjective, as *happiness*, *thankfulness*, and all nouns ending in "ness," the dictionary definitions are usually such as "the state of being happy, in any sense of that word," "the

state of being thankful," etc. These are words for which adjective and infinitive definitions abound with the children. The superiority of the dictionary makers, it will be noticed, is due to little more than a greater grasp of language.

The students' definitions stand half-way between those of the children and those of the dictionaries, their definitions are longer and more fully expressed, but do not exhaust the word. Philosophical, psychological and biological definitions are the most numerous sort. Many admit that the word called up definitions they had read when studying these subjects.

LITERATURE

Essai sur les passions, par TH. RIBOT. Felix Alcan, Paris, 1907. pp. 192.

This essay completes the series on affective states, of which "La psychologie des sentiments" and "La logique des sentiments" are the earlier volumes. The book contains four chapters, the first of which discusses the question, what is a passion, the second and third the genealogy of the passions, while the fourth is devoted to an analysis of how passions terminate. The author makes no claim of offering an exhaustive treatise on the passions but rather a monographic study based on modern methods of research. He thinks that the term passion has wrongly fallen into disuse in psychology and is needed to differentiate a distinct phase of affective life which otherwise must be classified under emotion, although it differs from emotion by distinct characteristics. As a convenient mode of classification Ribot groups the affective states into three classes: (1) Affective states or feelings, properly so-called, under which he includes all agreeable or disagreeable states which express the needs and appetites inherent in our psychophysical organism, which constitute the content of our ordinary every day experience. (2) Emotions, which are characterized by a sudden onset, a break in the equilibrium of ordinary consciousness. The author defines an emotion as a sudden reaction of the egoistic instincts, motor in its nature, involving always movement or arrest of movement and characterized by intensity and brevity. (3) Passion, which is a prolonged and intellectualized emotion. The latter is primary and crude, the former is secondary and more complex. It is characterized by the presence of a fixed idea, duration and intensity. Through the fixed idea, it involves association and dissociation, creative imagination and the logical function. Everything in harmony with the fixed idea is called up by association, everything inharmonious is dropped into the background of consciousness, thus preparing the way for the imagination, which idealizes the object of passion. The chief operation of the logical function is in the judgment of values. There are two types of passions, those in which the motor (affective) element is the stronger and those in which the relation is reversed. The latter are the more enduring.

The second section, which deals with the genealogy of the passions, is prefaced by the statement that although we have numerous works under the title, Physiology of the Passions, we, nevertheless, have no physiology of passions considered as special manifestations of the affective life, since a general knowledge of the physiological conditions underlying the affective life is entirely inadequate to explain the specific and necessary conditions of individual passions. Such conditions, especially in the more intellectual passions, must, of necessity, be exceedingly complex. The author, however, considers it necessary to distinguish two levels, one corresponding to the higher and the other to the lower elements of passion; the first to unconscious or subconscious factors, and the latter to conscious states. There are three fundamental tendencies in correspondence to which all passions may be classified into three groups — those which are directly connected with the conservation of the individual, gluttony and drunk-

enness; with the preservation of the species, love; and with the tendency of the individual to self-expansion (Nietzche's will to power). The birth of a passion is conditioned by both external and internal stimuli, only the latter of which can be considered as true causes. The outer stimuli are environment, which favors the development of the germinal tendency, imitation, and suggestion, which is really a form of imitation. The internal causes are the physiological constitution of the individual, his temperament and character.

Although there are many tendencies which express needs connected with the conservation of the individual, it is only out of hunger and thirst that actual passions can develop.

The physical basis of passion tending to the preservation of the species is a purely mechanical, conscious or unconscious, attraction between two individuals, whose normal form is a synthesis of homogeneous tendencies, and therefore enduring, while those forms of love which contain heterogeneous tendencies, as, for example, jealous love, are more likely to be unstable.

The passions which are based on the tendency to expansion or "will to power" are sub-divided into those based on sympathy, those which involve conquest and those which are destructive in their nature. The first form is productive of but few passions, of which the extreme type of maternal love is the complete example, and the author devotes but a short section to its discussion.

The second group is of more importance and includes many apparently diverse passions, gambling, ambition, avarice, etc.

The third group has, as its general basis, antipathy, and comprises the destructive passions, hate, of which vengeance is the final form of expression, and the different forms of jealousy.

There is, further, a group of passions which, though not universal, do not differ in their essential characteristics from the preceding groups. These are the æsthetic, the religious and the political passions.

The basis of the æsthetic feeling, Ribot finds in the instinct of play, here following the theory of Groos. The æsthetic passion arises whenever art is recognized as an absolute good. It is, according to Ribot, stronger in the amateur and art lover than in the creative artist.

The religious passion expresses itself in both active and passive forms, the latter being represented by mysticism and asceticism, and the former by missionary zeal and fanatical persecution. At the basis of the active forms lie in the one case altruistic tendencies, sympathy with unbelievers and overflow of energy, in the other antipathy toward the unbelievers.

The political passion is based on the social instinct, and there are two types, the realistic, in which personal ambition comes into play, and the idealistic, which is less egoistic, tends toward mysticism, and the passion resembles a moral imperative.

Patriotism may also be a passion but is less complex than the political passion and may even be opposed to it though it is grounded on the social instinct.

The moral sentiment may become a passion which expresses itself in two forms, namely, by propaganda and by action.

Besides these passions there is a group which the author designates as "petites passions," but these are so characterized because of the comparative insignificance of their objects since they are often as intense as the great passions. Such are bibliomania and the various forms of the collecting passion.

The fourth and last chapter of the book is devoted to the discussion of how passions terminate.

The author gives a brief outline of the development of passions, which vary in their details, according to the nature of each passion. But all passions are of slow growth and for the most part developed before they are fully present in consciousness. Without entering into any discussion of their unconscious or subconscious growth, which, however, Ribot regards as a probable hypothesis, there are present in consciousness fragmentary tendencies, scattered and perhaps of momentary duration, which are, nevertheless, all directed toward the same end, namely, attraction or repulsion for a person, object or idea. These movements of advance or recoil follow the law of nervous excitation and are cumulative in their action so that though passions sometimes seem to arise suddenly, the suddenness is only apparent. Through the accumulation all the various tendencies and judgments of value the passion is formed and this takes place as soon as a dominant idea recognized as such is present.

A passion may terminate in five ways: (1) by weariness or satiety; (2) by transformation into another passion having a common basis; (3) by substitution; (4) by insanity; (5) by death. Since physiological processes of stimulation underlie all passions and these, according to Ribot's views, are also present even when the consciousness is not ruled by the passion, it follows that the more intense is the physiological excitement the more easily can weariness or satiety occur. The termination of a passion indicates that the physiological excitability no longer exists. This may occur in consequence of physical weakness due to illness, exhaustion or old age. Habituation is not an essential element in passion. As an accessory factor it may be indifferent, useful or harmful. In a true passion the rôle of habituation is only an apparent value. Since the stability of passion has its root in the underlying tendencies of attraction or repulsion, its persistence depends not upon the nature of the passion but on that of the individual.

A passion may terminate by transformation into another which has a common basis. This is only an apparent end and is dependent upon two conditions, a surplus of energy, which has need of expression, or the appearance of a new directive idea. The most frequent examples of this type are the transformation of human love into love of the divine or religious into political fanaticism, or a passion may change to its opposite, as love to hate. Here the fixed idea does not change but there is an inversion of its value.

The substitution of a passion in place of one totally different happens but rarely. The problem of substitution is complex and has various aspects. In some cases the dominant passion appears to depend upon age, *e. g.*, the ruling passions in infancy are the nutritive, in youth love, in adult life ambition, in old age avarice. Again, there are men of a single passion, but more frequently men of many passions either co-existent or successive, and the dominance of one of these may give an illusory appearance of substitution or among the many tendencies a true passion may arise.

A passion may end in insanity, and the question naturally arises as to whether passion itself may not be a pathological state and how the fixed idea of passion may be distinguished from that of insanity. The chief mark of distinction in the fixed idea lies in the fact that in the abnormal form the idea may be not only undesired but oppressive and repugnant, while the fixed idea of passion is desired and cherished. In general, the author concludes that there is no one characteristic by which passion may be distinguished from insanity, but each case must be considered in its entirety. Every passion may end in death. Some, like gluttony and drunkenness, carry this tendency within

themselves. Others, like the passion for gambling, adventure and ambition, may lead to it, through external circumstances.

As a whole, the book is interesting, clear in its outline, and suggestive, as are all the works of the author, yet one is left with a wish that the general relation of passion to the affective life had received a more fundamental treatment and that the rich stores of material in biography and pathology had been drawn upon more extensively.

THEODATE L. SMITH.

Der Gegenwärtige Stand der psychologischen Forschung, von PROF. DR. C. GUTBERLET in Fulda. Philosophisches Jahrbuch, 21 Band, 1 Heft, S. 1-32.

This article follows the method and outlines of an earlier article by Stumpf, entitled "Richtungen und Gegensätze in der heutigen Psychologie," thus presenting the present status of psychology in the form of its numerous oppositions and conflicting points of view. These oppositions are of different degrees and vary greatly in importance. Briefly outlined they are as follows: An opposition exists between the psychological and the antipsychological or *a priori* points of view. According to the more moderate advocates of the psychological point of view, psychology is the foundation of only the mental sciences, but according to the stricter defendants it is fundamental to all science, and all judgments which are useful for life are by continuous habituation transformed into constraining propositions, and the *a priori* reduced a mental experience. Of somewhat less importance is the question whether psychology is to be considered a natural or a mental science and in direct relation to it, the question of whether a substantial substrate of mental activity must be assumed or whether this is an extraneous question and "psychology without a soul" answers all scientific demands.

Conflicting points of view also exist between Spiritualism and Materialism and between Substantialistic and Actualistic psychology, the latter being represented, not only by Wundt and his followers, but by all opponents of the doctrine of a substantial soul.

The advocates of psychophysical parallelism stand in opposition to the adherents of the doctrine of reciprocal action of mind and body. The parallelists as actualists can admit only states of consciousness as psychological material, since only these are actual while, on the other hand, some psychologists, of whom Lipps is representative, think that psychological processes lie more below than above the threshold of consciousness, only results of activities appearing in consciousness. Prof. Freud now believes that he has found an experimental method of studying the unconscious, which can be applied to hysteria and to dreams. Gutberlet thinks, however, that the expectations raised by Freud's method may be too optimistic, especially since the question involved is connected with Herbart's "freisteigenden Vorstellungen," which are now definitely set aside. Conflicting points of view also exist between Determinism and Indeterminism, and between purely Observational and Experimental psychology. Yet Wundt himself warns against an over doing of experimental methods and denounces especially such work as that of Bühler and Marbe who have sought to investigate processes of thought and judgment through questioning subjects and demanding self-instropection. These so called "Ausfrage" experiments he declares are no experiments at all and the observers have observed nothing.

Different methods of investigation also exist in Subjective and Objective psychology, the former being limited to introspection while the latter investigates the soul life of other beings and includes com-

parative, child psychology, and abnormal psychology. Here belong, also, hypnotic investigations.

More or less opposition in view-points also exists between Descriptive and Genetic, Pure and Applied psychology. During the last two decades the applications of psychology have been numerous and it has been brought into close connection with pedagogy, psychiatry, jurisprudence, national economy, art and language, research and theology. One of the most recent sensational applications of experimental psychology is the application of the association method in the service of justice. In the October number of McClure's, Prof. Münsterberg gives an account of his experiments with the criminal Harry Orchard. Cohnstaedt has, however, raised serious objections as to the validity of results obtained by this method.

In the more modern psychology are also certain oppositions dependent upon the different directions taken by investigation. These are the oppositions between Phenomenal and Functional psychology, between Nativism and Empiricism, Atomistic and Unitary, Voluntaristic and Non-voluntaristic, Apperception and Association, Normal and Abnormal psychology. THEODATE L. SMITH.

Die Cellularphysiologische Grundlage des Gedächtnisses, von MAX VERWORN. Zeitschrift der Allgemeinbiologie, Vol. 6, 1906, pp. 118-139.

Ueber die materiellen Veranderung bei der Assoziationsbildung, von Geh. Medizinalrat PROF. DR. GOLDSCHEIDER in Berlin. Neurologisches Centralblatt, Vol. 25, 1906, pp. 146-157.

Both Verworn and Goldscheider look upon "nerve-paths," whether of memory, association, or habit, as nutritive effects of functional exercise. In nerve-cells, as well as in muscle or gland-cells, the catabolism of exercise is followed by the anabolism of rest, which not only restores the cell to its original size and strength, but increases it somewhat. Within limits, the exercise of the function creates the mass of the cell. The millions of undeveloped cells in the brain would develop if only they were sufficiently exercised.

This increase in the mass of the protoplasm in the cell results in greater instability and in a correspondingly heightened power of functional discharge. A large cell discharges more powerfully than a small one for the same reason that a large amount of gunpowder produces a greater explosion than a small amount. Because of its greater instability, the large cell has a lower threshold and is therefore more easily discharged than is a small unexercised one.

At birth the cortical cells are yet embryonic. Sensory cells are the first to be exercised by incoming stimuli. In the beginning they lack the size and strength necessary to discharge into the adjacent cells with sufficient intensity to set off the latter. Exercise confers the power to break through the cell-separation and to discharge the next cell in the chain; its discharge causes it to grow through exercise until it also has acquired the capacity to discharge a still further one in the chain; and so on indefinitely. Thus association chains are formed. The "paths" which impressions leave in the nervous system are therefore only increased growth-effects in the exercised elements. The "path" is the line of least resistance through the developed cells of low threshold and high power of discharge. The impulse once started along the line cannot run off into other previously unexercised cells because of their higher threshold and their lower power to carry forward the impulse to further cells.

These "traces" are latent for consciousness so long as the cells are at rest; but if any stimulus whatever starts the chain of discharges,

these will occur in the way the cells have been previously exercised,—the same neural series, the same mental accompaniments. Lack of exercise leads to atrophy of nerve-cells just as it leads to atrophy of any other kind of cells. Forgetting is therefore a nutritive effect; unexercised cells finally become too feeble to break through the points of cell-separation with sufficient intensity to set off the next cells in the chain and thus arouse the memory.

So far Verworn. He is indisposed to admit any factors except those nutritive ones that apply to all kinds of living cells. He regards the greater instability of the protoplasm of the developed cells as the result only of greater mass with the consequently greater tendency to break down. He admits the possibility of changed chemical structure in the exercised protoplasm, but considers it problematical and unnecessary to an explanation of the phenomena.

Goldscheider lays more emphasis upon changed molecular structure. In this connection he makes use of Verworn's *Biogenhypothese* and Ehrlich's *Seitenkettenhypothese*. He also ascribes greater importance to the fibres. He thinks those fibres and portions of fibres that are exercised will acquire greater functional instability than the unexercised ones. An incoming impulse will therefore tend to discharge along those fibre-branches that have been rendered most permeable by exercise, and to avoid those more stable non-exercised fibre-branches. He makes much of the neurone-ends where the impulse passes across from one neurone to another, regarding the protoplasmic molecules of these regions as particularly unstable. His view may be illustrated as follows:

An object presented before the eyes, for example, simultaneously arouses a number of cells in the visual area. Where a fibre from one aroused cell is in functional contact with a fibre from another aroused cell, the activity in each fibre affects the catabolic changes going on in the other, resulting in a greater chemical disintegration in each fibre than would have been the case if the other had not been active at the same time. This greater catabolism leads in the following state of rest to greater anabolism, and, consequently, greater instability in these adjacent exercised fibres than in any of the other fibres which may have been just as near but which were functionally inactive at the time. Thus are formed associative lines of least resistance through fibres simultaneously stimulated (*Knotenpunktlinie. Kraftlinienresultante*). Frequent repetition of the simultaneous stimulations accentuates the effects and renders the lines of conduction more permeable and more permanent.

In the same way fibre-lines of low resistance and high powers of influence are formed by functional exercise between dissimilar sensory areas, between these and motor areas and the like. There may be many intermediate cells and neurone tracts. The one important factor is the nutritive result all along the line of high potential energy due to mass and molecular structure, and the low threshold of both cells and fibres due to a heightened instability in the protoplasm. At first the lines will possess unequal degrees of development at different points along their course; the result will be deflections, inaccuracies and error. Practice will have a cumulative nutritive effect, producing in time a uniform permeability in all parts with attendant ease, rapidity and precision. J. F. BOBBITT.

Die Mechanik des Geisteslebens, von MAX VERWORN. Leipzig, B. G. Teubner, 1907. pp. 104.

This little book gives the practically unchanged text of a series of five popular lectures on the 'mechanics of the mental life.' Lecture

I, 'Leib und Seele,' discusses the origin of the dualistic view of mind and body, and its seeming naturalness to the average civilized man; analyses the concept of outer 'reality;' and concludes with the formulation of a sensationalistic monism. "Es existiert nur eine unendliche Mannigfaltigkeit von Inhaltsbestandteilen der Welt, die sich gegenseitig in gesetzmässiger Weise bedingen." "Die Forschungsprinzipien können in letzter Instanz auf allen Gebieten immer nur die gleichen sein. Sie bestehen allein darin, für einen Vorgang oder Zustand, den wir wahrnehmen, sämtliche Bedingungen zu ermitteln." Lecture II treats of the conditions of mental process as shown in the action of stimuli upon the nervous system; it is entitled "Die Vorgänge in den Elementen des Nervensystems." A sharp line of distinction is drawn between the function of the cell-body and that of the nerve fibre. "Der Nerv leitet keine Lähmungsvorgänge, er leitet keine Narkose, er leitet keine Hemmungsprozesse, er leitet nicht das Absterben einer Zelle, er leitet nicht die Assimilationsvorgänge, er leitet nichts anderes als einzig und allein dissimilatorische Erregungen der Zelle, die so mit einer anderen in Verbindung setzt." The seat of sensation and idea, "das Wesentliche für den Enderfolg," is without any doubt the ganglion cell. Lecture III, "Die Bewusstseinsvorgänge," traces the doctrine of cerebral localization from Gall to Flechsig; gives the neural schema for sensation and voluntary movement; interprets the association centres as "Vorstellungszentren im weitesten Sinne;" and explains memory, the process of learning, on the analogy of muscular practice. Lecture IV, 'Schlaf und Traum,' reviews in unsystematic fashion various theories of sleep, and concludes that sleep is induced by a complex of conditions,—cellular fatigue, due to the continuance of dissimilative excitation by way of the nerve-fibre, and the removal of external stimuli. Dreams are states of partial waking, conditioned upon a local excitation (by internal or external stimuli) of the cerebral cortex. Lecture V, 'Suggestion und Hypnose,' presents hypnosis as a state of extreme suggestibility, "einen echten Wachzustand, in dem prinzipiell nichts anderes geschieht, als was im normalen Wachzustande passiert,"—a state of concentrated attention upon a single point determined by the operator. The conclusion repeats the position taken up at the outset. "Materie und Psyche, Körper und Geist, Leib und Seele, existeren für uns nicht als Dualität. Es existeren nur Dinge und Vorgänge von einheitlicher Art, ganz gleich, ob sie ausserhalb unseres Ich oder in unserem Ich sich finden. Ihre Analyse kann nur immer darin bestehen, die gesamten Bedingungen zu ermittlen für die Zustände und Vorgänge, die wir beobachten."

For the style of the book we have nothing but praise; the lectures read easily, and must have been delightful hearing. The contents are less satisfactory. The crude epistemology of Lecture I may be passed over. But the cells and fibres of Lecture II show us nothing whatever of the total mechanism of the nervous system,—the very thing, one would suppose, about which a popular audience would desire information. Lecture III is psychologically inadequate; mind is made equivalent to intellect, and attention and feeling and emotion are wholly ignored. In Lecture IV Professor Verworn has not availed himself of the latest and best results with regard either to sleep or to dreaming. The lecture on hypnosis is clear and interesting, but it runs altogether on the descriptive level. On the whole, while it may be readily acknowledged that the lectures did, and that the book will, do good, it must be said that the good is much less in amount than it might have been. Psychologists must continue to write their own physiological psychology, so long as the physiologists *von Fach* make no more of it than Professor Verworn has here done.

SAMUEL T. PRIOR.

Moderne Analyse psychischer Erscheinungen, by A. HOCHE. Gustav Fisher, Jena, 1907. pp. 17.

Hoche holds that there is too much reason to agree with the late Professor Möbius in believing that all psychology is hopeless. This applies with great force to that type of psychology that is chiefly interested in the problem of the relations of the body and soul, the reality of the objective world versus its subjectivity, the problem of the freedom of the will versus necessity, the doctrine of the immortality of the human soul. To the solution of these problems we are no nearer to-day than 2,000 years ago. On the other hand, there is very great need of another type of psychology. History, religion, criminal law, medicine, education are all almost crying out for more knowledge of psychic processes. We must, therefore, now expect a period of more special and above all more objective work. This is already begun in the laboratory where conditions for introspection are observed, also in psychopathology, in the study of children and of animals, and very specially, according to this author, in retrograde amnesia. The writer believes, therefore, that the old abstract psychology is at an end and that the new psychology, which is advancing just now so rapidly in the study of split personalities and psychic degeneration, variations from type, individual psychic pathography, and suggestion, possesses the future. Especially is he impressed by the new forensic psychology's association experiments called the Tatbestandsdiagnostik.

Attention, by W. B. PILLSBURY. Swan Sonnenschein & Co., New York, 1908. pp. 346.

This really is a new edition of a volume published in 1906 in French but the author has added chapters on the measurements of attention, its relations to feeling and the self and on the educational applications of some of the conclusions. He has also expanded a short chapter in the French edition on memory, will and reason into three, while new material has been introduced here and there throughout the book. It was certainly high time to have a comprehensive treatment of the theories of attention which are, as the author well says, at present in a chaotic state. The author has sought to give all explanations in terms of observed phenomena, to show critically the relations between fact and theory, and for the sake of completeness he places special emphasis on higher mental processes than is usual in works upon attention. The general plan of the book will be sufficiently indicated from the titles of the leading chapters which are as follows — the mental effects of attention; its motor concomitants, its conditions, interest and feeling of activity; its effects in consciousness; the methods of measuring it; its relations to ideas; association in perception, memory, will or action, reason, feeling or emotion; the self, its anatomical basis, its physiology, its relation to theories of apperception; history and critique of its psychological theories; attention in pathology and in development. The last two chapters deal with the general connections and applications to education.

Vom Fühlen, Wollen und Denken. Versuch einer Theorie des Willens, von THEODOR LIPPS. Schriften der Gesellschaft für Psychologische Förschung, Heft 13 and 14 (III Sammlung). Johann Ambrosius Barth, Leipzig, 1907. pp. 275.

The writer first discusses the idea of feeling, its relations to consciousness and to objects, its quality, the feeling of activity which he deems basal, endeavor, the conditions of pleasure. He then passes to striving and its verities, treating of its general essentials, considering it as inherited tendency of apperceptions, striving of energy or interest, its conditions, and finally of active, passive and assertive striving

or endeavor. In the third chapter he considers reality striving, which is tendency, consciousness of possibility and its analysis, normal and abnormal, the relations between it and between striving and the conception of an end or object and its actualization, apperception, realization of the goal of endeavor and the antithetic relations of unity. In successive chapters he considers positive, negative and passive endeavor, the conditions and the nature of energy involved and the possibilities of effort, the differences between instinctive and conscious effort, willing and physical activity, its relations to personality, will and judgment, the logic of will, pleasure, activity and worth.

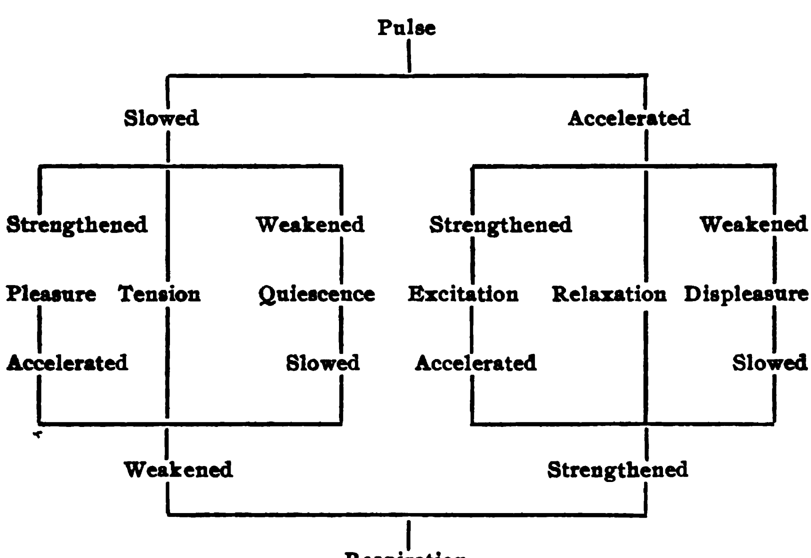
Die Grundformen der Gefühle, by N. ALECHSIEFF. (Aus dem psychologischen Laboratorium der Universität Sofia.) Mit 1 photolithogr. Tafel. Psychologische Studien, III Band, 2 und 3 Heft, 1907: 156-271.

Professor Alechsieff has performed a long series of experiments with a view to putting the various theories of feeling to an experimental test. Each experiment consisted of two parts,—an introspection by the subject and a careful measurement of the changes in the pulse-beats and the respiration by the experimenter.

His conclusions are as follows:

1. The best results can be obtained by applying both the impression and expression methods at the same time. In this way the results of introspection can be checked by the objective determination of the processes. But this plan cannot be used for the investigation of all the feelings, for many of the finer ones cannot be subjected to an objective test.

2. All psychical phenomena, which stand in no direct relation to the peripheral stimuli or the different sense organs and which are referred by us to the condition of consciousness, to the Ego, should be considered as feelings. The feeling processes are awakened by the



corresponding stimulus; they are, however, not merely connected with it; they express the changes in the condition of consciousness which arises on the entrance of the corresponding stimulus into the field of consciousness.

3. One does not meet pure and simple feeling processes. The mixed forms continually appear and the purely simple forms cannot be isolated.

4. The fundamental forms of feeling cannot be referred to two fundamental forms, such as pleasure and displeasure. They form a vast manifold and are grouped into three feeling directions, as Wundt has contended: *Lust-Unlust; Spannung-Lösung; Erregung-Beruhigung.*

5. The feelings, when they possess sufficient strength, are always accompanied by certain changes in the respiration and the pulse-beats. These changes serve as objective symptoms of the feelings. By the changes in the expression curves, six forms of feeling are determined.

6. Opposite changes in the expression curves correspond to the opposite feelings. They can be represented best by the foregoing scheme. p. 269. W. L. GARD.

The Doctrine of Primary and Secondary Elements, by DR. BORIS SIDIS, Brookline, Mass., Psychological Review, Vol. XV, Nos. 1 and 2 (January and March 1908).

Perception is treated from the standpoint of both the normal and abnormal mental life. The views of James Mill, Sully, Höffding, Taine and Wundt are objected to, on the ground that they compound perception from ideas or images. The fallacy of failing to distinguish between centrally and peripherally excited sensations is seen further in Titchener; Baldwin and James avoid this fallacy But the mistake of identifying the ideational and sensory processes is common and is to be traced back to Spinoza who made the image a weakened sensation and the sensation an intensified image. This theory is perpetuated through Hobbes, Locke, Hartley, Hume and James Mill to our times.

The author's idea with reference to the prominent and then the less conspicuous elements in perception is expounded by means of the relations of nucleus and cytoplasm in the organic cell. The slightest change in the prominent or nuclear elements brings about a considerable modification of the percept. A considerable change in the subordinate or cytoplasmic elements is needed to bring about a change in the percept. Consciousness plays with its searchlight on the nuclear sensory elements. The subordinate elements are indefinite, indistinct, in fact, may be even entirely subconscious, yet they form the main content of the percept giving it the fullness of reality. The percept is thus to be viewed as a compound whose elements are disguised and transformed by the qualitative aspect of the central elements. These central elements are, with their emotional and affective tone, the key to the situation. Biologically they are the trigger for the release of definite reactions and reveal the purposiveness of the percept.

The differentiation of the primary and secondary elements is seen in the *directness* or *indirectness* of the effects of the sensory stimuli upon the sense organs. In seeing an object other sensory elements are experienced beside the visual. These are not memory images; they have the same sensory characters as the elements given by the direct impression of the sense organs. The central sensory elements are *primary*, the subordinate are *secondary*. That these secondary elements are not ideational is seen from the differences between the ideational and perceptual. These are four: (a) A sensation has intensity, an image totally lacks it; (b) the image is a reproduction or

rather a representation of a sensation, but no sensation represents another; a sensation is an immediate experience; (c) a sensation bears the mark of externality, an image lacks it; (d) a sensation cannot be called up at will, while an image is independent of peripheral stimulation and is usually under the control of the will. Hallucinations are made up of sensory elements, while hypnotic hallucinations are made up of images. The muscular and tacto-motor sensations appearing as visual, when an object is seen, are not memory images, they are actual sensations, *secondary* sensations, giving fullness of content to the percept, having visual sensory elements as its nucleus. Pathology confirms this view. In certain mental diseases the patient can perceive but he cannot ideate. In others he can ideate but not perceive. Clinical cases point to the qualitative differences of image and sensation, irrespective of the assumption of localization. There are no memory images in perceptual consciousness although the latter may be closely associated with the ideational processes. Put briefly: "the external excitation acting in a particular sense organ produces its appropriate sensations but the peripheral physiological process diffuses, or rather to say, gets irradiated along other neurons of other sense structures, awakening their appropriate sensations. Such sensations, not being directly, but indirectly peripherally initiated should be regarded as secondary sensations." That secondary sensations are sensory and not ideational is seen from the fact that they can be produced only by a stimulus and by their own specialized peripheral physiological processes. One sensation always calls forth only a particular sensation and no other one, and that of a qualitatively different domain. "The main content of the percept consists of hallucinatory secondary sensations. Percepts and hallucinations are of the same grain. A percept is an hallucination with the primary nuclear sensory elements present; an hallucination is a real percept with the primary sensory elements absent." The dissociation of the secondary sensory elements from the primary elements causes us to regard the former as central phenomena — or as abnormal — but it is only the dissociation which is abnormal. When appearing isolated secondary sensations are the simplest form of hallucinations, which become more and more complex as the secondary elements dissociated from the primary became manifested in complex systems. Secondary sensations though present in every percept rarely appear in isolation. The affinity of secondary sensory elements to run into compounds becoming synthetized with primary elements makes it difficult to observe them except in synæsthesia and in the abnormal states of hallucination. Cases of visual and auditory hallucinations are cited. These are cases of the dissociation of the primary and secondary elements. This dissociation is often so deep and extensive that the synthetized system of secondary elements does not bear the least trace of the qualitative aspect of the primary elements; thus a morbid condition of the pharynx may give rise to an auditory and even to a visual hallucination.

The whole discussion is clear, and while perhaps in some minor points lacking in discrimination, is exceedingly suggestive, and will prove valuable in opening up some new lines of treatment.

E. E. Weaver.

Ueberblick über die Geschichte und den gegenwärtigen Stand des psycho-physiologischen Problems der Augenbewegung, by R. Herbertz. Zeits. f. Psy., 1907, Vol. 46, No. 2, pp. 123-141.

This is an eminently successful attempt to present in succinct form the historical development and present status of the problem of eye-movements. The author first points out that the problem is not

merely a psycho-physiological one, but that the theoretical question as to the manner in which the phenomena of the external world become presentations in consciousness also is involved. This opportunity for emphasizing one factor or the other is the reason for the various methods of approach and the different answers given by investigators. Ophthalmologists, attacking the problem from an anatomical-physiological point of view, established such laws as those of Donders and Listing. The search for some general principle led inquiry in a physiological-mechanical direction and to Wundt's principle of simplest innervation. Helmholtz thought that the really deciding factor in the problem is to be looked for in an optical principle and encouraged research which proceeded from the psychological conception involved in Meissner's principle of orientation. Earlier investigators, however, erred in attempting to establish a law or principle of eye-movements, rather than to inquire into their import for visual perception. Only by focusing on this, and by specializing research in connection with reading, has a definite and clear answer become possible. The fact, established by Erdmann and Dodge, that visual recognition takes place exclusively during the pauses of rest, while the eye-movements, properly speaking, are interfixation movements, represents a psycho-physiological solution of the problem. It was reached by ascertaining the time necessary for single ocular movements. Volkmann, Huey, Lamanski, Dodge and Cline experimented with this point in view. Dearborn, however, who perfected the method of photographic registration, was able to show that the average time of a single movement is probably 0.02 sec. or somewhat less. This brief duration, in view of Plateau's results, naturally precludes the possibility of our distinguishing between black letters and white spaces while the eye moves.

Since the question of the function of eye-movements in reading is but a specialization of the more general problem of their function in visual perception throughout, we can almost reverse the commonly accepted theory of the past and now say: Seeing, while the eye moves, is scarcely of importance for visual recognition; whenever we really visually recognize, the eye usually is at rest. (Obviously a guarded statement which implies the belief that the last word in the matter has not as yet been spoken.) M. W. MEYERHARDT.

An Experimental Study of Visual Fixation. PROF. RAYMOND DODGE. Studies from the Psychological Laboratory of Wesleyan University, Vol. I, No. 1. Issued by the Psychological Review, as Monograph Supplement of November, 1907.

Prof. Dodge first observed the movements of the eyes during supposed fixation, with the movements of head and body to which these eye-movements are in part compensatory. The eye-movements, however, are found to be due in part to *irregular* movements of head and body, and are then disturbances of fixation for which there can be no compensation. The pulse and breathing are important factors in producing the fixation movements. Head movements may be demonstrated by watching the reflection of objects from behind as seen through smoked glasses upon cross-section paper. Satisfactory methods of recording the movements have not been worked out. The compensatory eye-movements are united with the movements of head and body "into a thoroughly organized motor system," furnishing a co-ordinating mechanism "capable of explaining the intimate correspondence between tactual and visual space." There are also visual motives for the fixation movements, in retinal fatigue and in the correction of inadequate binocular co-ordination.

Control of fixation movements involves ocular reactions, and these

are again found to be slow, but a minimum of 130σ was reached. The method of taking the reaction times and making the exposures was that of Prof. Holt, with the alternating arc light, the stimulus being given by the fall of a screen which simultaneously threw the actinic beam upon the cornea, to be reflected to the falling plate of the camera. The alternations of the current gave an approximately accurate time-record in a series of dots, and this might be made fully accurate by interrupting the direct current with a tuning fork. The arc light was "stopped down" to comfort, by plates of pot blue glass, without materially lessening the effect upon the plate.

Repeated fixations of the same word gave very different locations of the point of regard, but all are "perfect fixations" when the "object of interest is brought to a retinal area of clear vision," the functional centre of the retina varying in size with circumstances.

Prof. Dodge calls a fixation "adequate" when it is long enough and accurate enough to condition a "clearing-up" of the perception of the object of regard. He measured the time needed for the clearing-up of words exposed on various pre- and post-exposure fields such as may occur in reading, and concludes that "the shortest adequate fixation pauses in reading are between 70σ and 100σ." But words exposed peripherally, in reading for example, may modify the total consciousness without clearing up, and he investigated the effects of such factors, finding the movement to be from general to special effects, phrase, sentence, and paragraph, episode and plot forming a "dynamic background" for each new word-complex as it clears up.

The article contains a reproduction of a photographic record of the eye-movements in fast reading, taken with the alternating current. The "overshoots" of the eye, shown in the reviewer's records, are confirmed but not yet explained. The shortest fixation pause occupied 40σ.

Prof. Dodge thinks that to explain the apperception processes which condition reading we must look at the "concurrent complication of psychological processes of perception extending through several fixations." To study these he used the exposure apparatus previously described, and measured the effects of pre-fixational perception of peripherally placed words. It is unfortunate that the author does not state the distance of these stimuli from the eye, so that the peripheral angle might be known, but the effects are evident and are such as are to be expected.

Prof. Dodge fails to find experimental evidence for the traditional theory of retinal local signs, and raises the question whether the spatial relations of the total visual field are determined by its relations to the fovea, or whether the object of regard is not rather "determined in its spatial relations by its apparent position in the total visual field." He proposes a substitute theory of "genetic organization of the retinal elements" which is hardly capable of brief statement and criticism.

The appendix to the article contains a timely review of the methods and technique of recording eye-movements by photographic registration, and describes the Wesleyan apparatus.

Professor Dodge has in some way fallen into an unfortunate error as to the size of the foveal and macular fields of regard, stating these as less than one-fifth their actual size as calculated by the present writer upon the data given by Helmholtz and Köllicker. This mistaken notion of the relative sizes of the central and peripheral fields has evidently intensified somewhat the zeal of the author in some of his contentions. But the present writer's own experiments upon the effect of peripherally given stimuli corroborate Prof. Dodge's

conclusions as to the large part played by extra-foveal if not extra-macular vision, in reading at least; and probably the error has not invalidated the main contentions of the article, whose positive contribution is important, and whose author continues to show his clever originality in experiment. EDMUND B. HUEY.

Das Behalten und Vergessen bei Kindern und Erwachsenen nach experimentellen Untersuchungen, von PAUL R. RADOSSAWLJEWITSCH. Bd. 1, Otto Nemnich, Leipzig, 1907. pp. 197. (Pädagogische Monographien hrsg. von E. Meumann.)

The author was a pupil of Meumann under whose direction this work was done. After giving an historical account of previous work in this field, the writer proceeds to experiments of his own; first on adults and then upon children. He finds that memory is more persistent for meaningless material than for that which has a meaning. The number of repetitions necessary both to learn and to relearn diminish with time, but the growth of practice is far greater in learning than in relearning. Even associations improve by practice as does the certainty of reproduction, provided the fidelity of the first impression remains constant. Learning and retaining are two very distinct processes of memory and have their own laws and conditions. The learning of meaningless material is of course far harder. A special practice in memory improves memory in general. As to forgetfulness, it begins very rapidly after learning and then its curve sinks more slowly. The initial loss is not nearly so rapid as Ebbinghaus thought. The types of memory differ greatly, although this seems to depend partly upon practice. The last and the first syllables of a series are least and those in the middle most forgotten. There are distinct slow and fast types of learning, although the former may by practice approach to some extent the latter, so the difference may be due to practice. Those who learn slowly retain better. It was a great help to each to follow his own type of memory with which few seem to be acquainted. All persons experimented upon were of mixed type, chiefly either visual-acoustic or acoustic-visual. Next came motor-acoustic and acoustic-motor. There was no visual-motor or motor-visual. The longer and harder task was given memory, the more sense elements were used, and only in very short series of syllables was there anything like a pure type. The impulse toward the sense of what was learned repressed the mere sensuous material. Nearly all, at first, tried to devise some logical connection between syllables; but with more practice, the more mechanical became the process. All are inclined to use rhythm and tempo, trying many at first and focusing later to a few. There was no indication that people of different nationalities preferred special rhythms. Adults exceeded children only when the work was prolonged, but adults need less repetitions. Neither showed any pure concept tpyes.

The Influence of Bodily Posture on Mental Activities, by ELMER ELLSWORTH JONES. Columbia Contributions to Philosophy and Psychology, Vol. XVI, No. 2. New York, The Science Press, October, 1907. pp. 60.

From the results of various tests in many series, the author found that the following activities were best performed in the vertical position. They are . . . the discrimination of pitch and number of taps per minute, the strength of grip. The following were best performed lying down . . . tactile discrimination, visual and auditory memory tested both by rapidity and fewness of errors and adding. The subjects showed greater signs of fatigue in the horizontal than in the vertical position.

Die Physiologische Methodik zur Erforschung der Tierpsyche, ihre Möglichkeit und ihre Anwendung, von GEORG FR. NICOLAI. Privat-dozent an der Universität, Berlin. Jour. für Psychologie und Neurologie, Oct., 1907. Band X, Heft 1 and 2. S. 1-27.

This paper describes a new method of research in comparative psychology, devised by Prof. Pawlow, Professor of Experimental Medicine in the University at St. Petersburg. The author spent three months in Pawlow's laboratory and has continued the experiments by this method.

The first point discussed by the author of this paper is the "psyche" as a concept of natural science, which he concludes is allowable, but must be strictly limited in its meaning to "the postulated ground of the actions of subjects, which is related to our subjectively felt psyche just as any form of mechanical power is related to muscle power as felt."

The conditions of method in psychology as a natural science are then enumerated. These are universality, constancy, measurability, and specificity. Any bodily function may be taken as an equivalent expresson of the psyche, but it must be independent of other influences and especially of the will. Such functions, then, as can be chosen for experimental purposes are blood pressure, pulse, respiration, separation of secretions or similar functions. Numerous experiments have been made with the first three, but the results have failed in the character of specificity. The method of Pawlow is the first to fulfill the four conditions just enumerated and he expressly states that his method is a physiological one and carefully avoids any expressions which might imply that his researches are on mental activities. The reaction chosen by Pawlow was the secretion of saliva and all his experiments were carried out on dogs. The dogs were operated upon in such a way that a salivary gland, *e. g.*, the parotid, was given an opening on the outer side of the cheek. After a few weeks this healed, leaving a permanent outlet, which appeared in no way to disturb the animal or to cause him any inconvenience, so that the reactions may be considered as normal. In order to measure the quantity of the salivary secretion, a glass tube was attached to the opening in the cheek. This very simple method gave a record of the quantity, but not of variations in the rapidity, of the secretion at different instants of the experiment, and a modification of this was, therefore, adopted, which by means of tambours, registered upon a revolving drum, so that both the rapidity and quantity of the salivary reaction were shown graphically, Saliva flows not only when anything edible is brought in contact with the mouth, but also if this is smelled or seen. The ordinary salivary reflexes from contact are practically constant and unchangeable, but under certain conditions, essentially different reflexes appear which are not constant. Pawlow had already demonstrated earlier that whenever saliva flows there is some reason to be found for it in the environment, and this in a sufficient number of cases to make it practically certain that with sufficient knowledge of the condition this reason could be found. These irregular reflexes Pawlow designated as conditioned reflexes. They differ in two points from the unconditioned reflexes: first, they may, under certain circumstances, be excited by any sense impression, and secondly, they are extremely inconstant. If something is placed in a dog's mouth, saliva will flow in all cases, but if food is only shown him or he is allowed to smell it, the salivary reflex may or may not occur. The next problem is to find out under what conditions such reflexes occur and whether they can be experimentally controlled. Although all the laws of salivary secretion are not known, certain laws of the appearance and disap-

pearance of the conditioned reflex have been established in the St. Petersburg laboratory. They are as follows: Every conditioned reflex can be destroyed by repetition. The shorter the time between its repetitions, the quicker the reflex vanishes. The destruction of a conditioned reflex in no way affects other conditioned reflexes which may be present, *e. g.*, the nullifying of a conditioned sight reflex does not affect one of smell. In cases of the vanishing of the reflex, it re-establishes itself after some hours, or, in cases of very frequent repetition, after some days. There is, however, a possibility of re-establishing it immediately by allowing it to become effectual. Thus if sight and smell reflexes to a dry meat powder have disappeared, they return as soon as the animal has been allowed to taste it.

By means of this conditioned salivary reflex a wide range of experiments becomes possible. From the nature of these experiments great care to avoid sources of error is necessary. Since anything in the environment can, under conditions, become the occasion of a conditioned reflex, it is necessary in all cases where experiments are made with conditioned reflexes to have an acute and well trained observer who is capable of distinguishing whether disturbances came from the environment or are due to voluntary or involuntary movements on his own part. Pawlow has carefully taken into consideration these sources of error and impressed upon his students the necessity of care and foresight in the manipulation of all the technique of the experiment, that the dog, which is the subject of the experiment, may not be disturbed by them.

By this method of experiment with due consideration of the possible sources of error the following results have been obtained. It is possible to decide what details of the outer world can become excitants of the dog's brain. In order to test this, any conditioned reflex may be aroused by any stimulus and the maximal change which can be made without destroying the reflex determined. When, *e. g.*, the application of cold to a certain portion of the skin the size of a dollar acts as a conditioned reflex, the saliva flows and the same result occurs when the cold is applied to another circumscribed place. This shows that the cold stimulus is a generalized one over the whole skin and that the reaction of the dog to cold is not finely graded according to the place stimulated. The same is true of heat stimulus. A totally different result is obtained if a mechanical stimulus, *e. g.*, tickling is used. When by the application of a mechanical stimulus to a specified place the reflex is aroused, it may be entirely without effect upon another portion of the skin, thus showing that the dog's reaction to tickling is more finely differentiated according to place. Hard and sharp objects are also clearly distinguished. The analytic capability of the dog's nervous system for acoustic stimuli is also great. It is comparatively easy to accustom dogs to react to a tone of special pitch and quality and not to other tones. It is then also comparatively easy to establish that the absolute hearing of dogs and their musical memory far surpasses that of most human beings. Many dogs — not all, for they vary individually as do human subjects — are able, after days, to recognize a tone which is only one-quarter a tone higher or lower than one previously associated with feeding. Furthermore, dogs also clearly hear this tone in a chord. The author by the expression 'recognizes a tone' does not mean to imply that any process of recognition takes place in the consciousness of the dog but only that different reactions follow different tones, and that, therefore, the dog must possess an organ which distinguishes tones. For instance, the dog may react to a tone associated with feeding by a secretion of ten drops of saliva, to one-quarter of a tone higher by eight drops, to one-

half of a tone higher with four drops, and to a whole tone higher with only one drop, while at a greater interval the reaction entirely vanishes. At a third or an octave he again reacts.

The method was also applied to optic stimuli and tests made of the dog's ability to discriminate form and color. A dog which six months earlier had been used for other optical experiments but had not been experimented upon in the meantime was shown several times daily, when fed, a shining white circle of about 15 cm. diameter, at a distance of 120 cm. This was continued for a month. After the association was thus established he was occasionally shown the circle without being fed, though the regular association with feeding was continued. He was also on the thirty-sixth day, without being fed, shown a square and later a pentagon. At first there was but a slight difference in the reaction to the square and the circle when shown without feeding, but gradually the reaction to the square lessened and on the fourth day practically disappeared, and the reflex to the pentagon which had been shown only twice, also disappeared at the same time. A heptagon was then used and the number of days required for the disappearance of this reflex was considerably greater than in the case of the square and pentagon. When, however, a hexagon was used there was, from the first, no reaction. In this case, the dog seemed without previous experience to refer this form to the no-food group of figures. Pawlow states that he has experimentally established by this method the capability of color discrimination in dogs. Nicolai, however, obtained only negative results in his experiments carried out in the Berlin Physiological Institute, the dogs becoming easily confused when red and green of different degrees of brightness were shown them. He would not, however, on the ground of these experiments dispute Pawlow's results, since there are numerous possibilities of error.

Other experiments have established that the analytical capability of the dog may be heightened in the case of sensations that can be measured. In these experiments temperature and mechanical stimuli were used, *e. g.*, a quick rubbing of the skin (60 strokes per minute causes a larger quantity of saliva to be secreted than a slow stroking (20 strokes per minute). This holds good not only for mechanical and temperature but also for optic and acoustic stimuli. Comparative experiments with the different senses have also been made, *e. g.*, with temperature, mechanical and optical stimuli from which it appears that the tickle stimulus is much stronger and has more significance for the dog's existence than temperature stimuli; also that mechanical are stronger than optical stimuli. Furthermore, newly introduced stimuli may have an inhibitory influence. When, for instance, a new stimulus of the same kind as one already acting is introduced there is an inhibition of the first, *e. g.*, reaction to one odor may be inhibited by the introduction of a second odor, or the introduction of a tone simultaneously with one already acting as stimulus may inhibit the reaction.

The possibility and advantages of this method are easy to discern and Pawlow has made a most important contribution to comparative psychology, for the introduction to which psychologists must be grateful to Dr. Nicolai, as the Russian reports are inaccessible to most readers.

THEODATE L. SMITH.

The Dancing Mouse, by ROBERT M. YERKES, PH. D. The Macmillan Company, New York, 1907. pp. 290.

Animal psychology is rapidly passing from the stage of chance observation to the stage of careful experimental investigation. Dr. Yerkes's book is an unusually valuable contribution, and may well

serve as a model for similar studies of other animals. The value of the book consists as much in developing and demonstrating experimental methods as in the results and conclusions reached concerning the dancing mouse. The entire work is characterized by exceptional care and accuracy in planning the tests and interpreting the results.

The first four chapters deal with the history, care, and general behavior of the dancer. "The three most clearly distinguishable forms of dance are (1), movement in circles with all the feet close together under the body, (2) movement in circles, which vary in diameter from 5 cm. to 30 cm., with the feet spread widely, and (3) movement now to the right, now to the left, in figure-eights." "There are three kinds of dancers: those which whirl almost uniformly toward the right, those which whirl just as uniformly toward the left, and those which whirl about as frequently in one direction as in the other."

The next two chapters are concerned with hearing. Previous investigators disagree as to whether the dancing mouse can hear. Yerkes shows by direct and indirect tests that adults, *i. e.*, mice more than five weeks old, are totally deaf, while some of the young give evidence of ability to hear from the 13th to the 19th day of life.

Chapters VII to XI deal with sight. The experiments on this sense are particularly valuable because of the new devices and methods employed. In regard to brightness-vision, the tests demonstrate that the dancer perceives a difference between black and white, and even between shades of gray. An attempt was then made to determine whether brightness discrimination obeyed Weber's law. Only one animal was used here whose ability to discriminate brightness differences gradually improved until a difference of one-tenth seemed distinguishable in the case of three standards of brightness. Weber's law probably holds. More data are needed, and, as the author states, the study of this problem is merely begun.

The experiments on color vision emphasize the importance of check tests and of eliminating brightness from color discrimination. "Although the dancer does not possess a color sense like ours, it probably discriminates the colors of the red end of the spectrum from those of other regions by difference in the stimulating value of light of different wave lengths, that such specific stimulating value is radically different in nature from the value of different wave lengths for the human eye, and that the red of the spectrum has a very low stimulating value for the dancer."

Chapters XII to XVI deal with the educability of the dancing mouse. The experiments were made according to three methods, the problem method, the labyrinth method, and the discrimination method. These are critically compared in regard to their merits in the investigation of various problems. The main results are that the dancer "does not learn by imitation to any considerable extent," and "that it is aided by being put through an act." Training in one form of labyrinth facilitates the learning of other labyrinths.

Chapter XVII is devoted to differences in behavior. "The race exhibits individual differences in discriminating sensitiveness to a far greater extent than do most mammals."

In the last chapter, dealing with the inheritance of forms of behavior, the conclusion is reached that there is no evidence of the inheritance of the individually acquired habit of discrimination between black and white. The experiments were carried through four generations only.

DANIEL STARCH.

Wellesley College.

The Behavior of "Roger," by ROBERT M. YERKES. The Century Magazine, Vol. LXXV, 1908, 602-608.

Mr. B. B. E. was successful in training a mongrel to perform many clever acts, an account of which appears in the same issue of *The Century*, pp. 599-602. The dog was able to spell such words as "Constantinople," "phthisic," and "pneumonia," and solve problems like "$2 \times 3 + 4 \div 2 - 1$," and "never make a mistake."

Professor Yerkes brought the trainer and the dog into the laboratory, and was able to come to the following conclusions concerning the dog's method of procedure:

1. "Simple associations of certain objects with definite acts."
2. "The habit of watching for slight movements of the eyes, head, extremities, and body of the trainer, and of making movements which experience has proved to be advantageous."
3. "The association of certain tones of the trainer's voice and certain facial expressions with definite forms of behavior, such, for example, as begging, praying, being a dead dog, and whispering."

W. L. GARD.

The Animal Mind, a text-book of psychology. By MARGARET FLOY WASHBURN. (The Animal Behaviour Series.) The Macmillan Company, New York, 1908. pp. 333.

The title of this book, the author says, should have been the animal mind as deduced from experimental evidence, for she has confined herself chiefly to the results of the experimental methods in comparative psychology. Thus, many aspects of the animal mind to the investigation of which experiment either has not yet been applied or is perhaps not adapted, are left wholly unconsidered. In this respect the book is new, for no other has yet limited itself to this field. The value of it is greatly increased by a bibliography of 476 titles, mostly, though not exclusively, limited to the scope of experimental results and methods. Chapter first is devoted to the difficulties and methods of comparative psychology. Then come the evidences of mind in the simplest animals, sensory discrimination, methods of investigation, hearing, vision, reactions and space perception, modifications of conscious processes by individual experience, the memory, idea and some aspects of attention. This work has barely a score of simple illustrations, and altogether it is almost a Godsend to the student and teacher of psychology at the present day.

An Introduction to Comparative Psychology, by C. LLOYD MORGAN. 2d rev. ed. With diagrams. (Contemporary Science Series.) Charles Scribner's Sons, New York, 1906. pp. 386.

In this book the writer sums up his own views upon the subject so well that it may be said to supersede all his previous writings. He attempts here to give them more unity and discusses a number of general psychological problems which he has not touched before. The chief topics are: — the wave of consciousness; its physiological synthesis and correlation of impressions; instinct and intelligence; the sense experience of animals; automatism and control; perception of relations by men and animals; whether the latter reason; concepts; subject; object; the evolution of consciousness; selective synthesis in evolution; the psychology of man and higher animals compared.

The Story of Insect Life, by W. PERCIVAL WESTELL. Robert Culley. London, 1907. pp. 339.

The writer deals with his facts in an interesting, informing and

orderly manner, confining himself to the commonest species of British insects and to a style intended to encourage intelligent life study of them by younger people, to discourage collecting, but to stimulate the profitable employment of eyes and ears in town or country. The insects chiefly treated are: beetles, earwigs, cockroaches, crickets, grasshoppers, dragon-flies, may-flies, lace-wing flies, ants, bees, wasps, gall-flies, butterflies, moths, bugs, frog-hoppers, gnats, crane and other flies.

Mosquito Life, by EVELYN GROESBEECK MITCHELL. G. P. Putnam's Sons, New York, 1907. pp. 281.

Although very much has been written about mosquitoes in recent years, it is widely scattered through many books and periodicals, and until now there has been no single work containing in condensed form the essential facts made known concerning the different phases of this important and interesting topic. The writer first describes the systematic position and structure of the eggs, larvæ, pupæ and imago and then some adult habits, such as blood sucking, diet of males and females, hibernation, how long mosquitoes live, how far they fly, etc. Then she discusses malaria, yellow fever and other diseases. The work contains 54 illustrative diagrams.

The Life History of the Carpenter Ant, by JOHN LOSSEN PRICER. Biological Bulletin, Vol. XIV, 1908, 177-218.

Two varieties of *Camponotus herculaneus—C. pennsylvanicus*, and *C. ferrugineus*—were studied. He finds that winged forms are not produced until the colony is more than two years old. Sexually perfect individuals are not produced until the colony numbers approximately two thousand workers. It requires a colony from three to six years to reach this size. He believes that the variations in form are ontogenetic in origin and that there is no distinct soldier type. The division of labor among them is also incomplete. The ants show a decided preference for the red or longer rays and a decided dislike for the ultraviolet rays. In all probability the light is perceived through the eyes. These ants have some means of inter-communication. They can track themselves and others of the colony, but are not able to make out the direction in which the trail was first laid down. They seem to be guided by a kind of memory of the location of things and perhaps depend, as a last resort, on a sense of direction. When travelling from the nest, they usually pay very little attention to trails. They give no evidence of anything akin to reason. W. L. GARD.

Behavior of the Starfish, Asterias Forreri de Lorial, by H. JENNINGS, The University of California Publications in Zoölogy. Vol. 4, No. 2, pp. 53-185. Nineteen text figures. Nov. 26, 1907. Contributions from the Laboratory of the Marine Biological Association of San Diego.

The chief result developed by this careful paper is the demonstration of the variability, modifiability, unity and adaptiveness in the main features of the behavior of the starfish. The movements are shown to depend on the varying physiological conditions of the animal and the animal's nature, which demonstrably modify the physiological condition, and therefore the behavior are set forth in detail. Habit formation is demonstrated and discussed in full. The unity and co-ordination of much behavior is shown, with certain theories of its origin. There are also essentially new accounts of the method of locomotion. The monograph is intended to be a storehouse of objective facts for reference concerning the starfish so that the author deems it hardly practicable to make out a summary. He has provided

an admirable index. This work is now practically a thesaurus of what is known upon the subject.

La Nature et la Genèse des Instincts d'après Weismann, by E. MAIGRE. L'Année Psychologique, Vol. XIII, 1906, 230-244.

In this article Maigre has given an exposition of the theory of instincts which appears in Vol. I of Weismann's The Evolution Theory, 1904, pp. 141 *et seq.* (English Edition).

For Weismann instincts have a physiological basis in the cells and fibres of the nervous system. They vary and thus become subject to the operation of natural selection. But Maigre points out some instances where Weismann's theory fails. He cites the case of the kitten of Lawson Tait, and the skye-terrier of Hurt. He further reminds the reader that much comparative work is needed on this subject and believes that in the end we shall have to go back to protoplasm for the basis of an explanation of instinct.

W. L. GARD.

L'Inhibizione Motrice: studiata sperimentalamente negli ammalati di mente, by ETTORE PATINI. Ed. "Il Lavoro Internazionale Illustrato," Napoli, 1907. pp. 256.

This contribution to experimental psychology is dedicated to Prof. Leonardo Bianchi, with whom the author has worked. It is a comprehensive study of inhibition including a review of the various theories and the forms under which it appears as physiological, cerebral, psychic and psycho-somatic inhibition. The most interesting portion of the book, about one-half, is that devoted to the author's own experiments upon both normal and insane subjects. Libertini (1895) studied cortical inhibition in dogs by comparing the reflex reaction time of normal animals with that of dogs in which portions of the brain had been removed. He found that extirpation of the left frontal lobe diminished the time of the reflex of the fore leg, and that the same result, in a lesser degree, followed from extirpation of the occipital lobe. Fano, from similar experiments, came to the conclusion that the cerebral cortex exercises a tonic inhibitory action upon the spinal cord. Libertini also experimented on the reflex reaction time of the insane and reached the following conclusions:

In all forms of mental maladies the reflex time of arm movements is noticeably shortened.

In normal individuals this is a constant which oscillates between 83σ and 86σ. This diminishes in different forms of mental disease proportionally to the gravity of the affection and the degree of the patient's mental decadence.

In general, forms of exalted insanity show a greater reduction of reaction time than those of a depressive nature.

It is possible experimentally to reduce the latent time of the spinal reflex both in normal individuals and in the insane, the possible reduction being twice as great in the former as in the latter.

Patini's experiments differ from the preceding in the introduction of a new element and an inversion of the problem. The previous experiments have tested the influence of brain upon movement, those of Patini are directed to finding the effect of voluntary movement upon the brain and therefore upon the inhibition exerted by it upon voluntary movement.

His method of experiment was as follows: The subject was seated between two tables, upon one of which was arranged the apparatus necessary for obtaining an accurate record for the time of reflex reaction of the left arm to an electric stimulus. This consisted of a Hipp chronoscope, a Dubois-Raymond induction coil and a special inter-

rupter devised by the author. The right arm of the subject was placed at rest upon the other table with the elbow bent at an angle of 45°. In this position the muscles were contracted sufficiently to support a weight of 5 or 10 kilos, according to the strength of the subject. This position was maintained for some seconds, on an average about forty. This gave a static contraction of the right arm, *i. e.*, a contraction in which no movement takes place but the resistance of the weight is just counterbalanced. During this contraction, the time of the reflex contraction of the left arm to electric stimulus was taken. When fatigue began to set in, as was indicated by the trembling of the arm sustaining the weight, the reflex time of the left arm was again taken, and again after the release of the right arm from the weight. These results were compared with the simple reflex time without the static contraction, thus giving four series of reactions. These series were obtained from 12 normal subjects; young men from eighteen to twenty years, and from 68 subjects suffering from different forms of mental disease. In all but one subject, the reactions of the mentally diseased were shorter than those of normal subjects. In this case, a paralytic dement with progressive epileptic attacks, the reaction time was longer than in any of the normal subjects, thus showing an excess rather than a defect of inhibition. Patini's results agree with those obtained by Libertini in showing a progressive shortening of the reflex time according to the degree and form of the mental disease, but he attaches only a relative value to these results. In the reflexes of the left arm during the voluntary muscular contraction of the right, there was found to be a lengthening of the time, that is to say, the voluntary contraction had an inhibiting effect.

Fatigue increased the variations in the individual reactions. In general, the results of Patini's experiments show that the effects of voluntary contraction, of fatigue and of rest after fatigue in mentally diseased patients differ from those of normal subjects only in degree. The increase of inhibition which was shown by a lengthening of the time in the experiments involving voluntary muscular contraction, was, with the one exception already mentioned, less in insane than in normal subjects. The mean variation of the individual reaction times was, however, greater in insane subjects.

The study as a whole is an interesting contribution to the study of inhibition. THEODATE L. SMITH.

Contributo allo studio sperimentale della formula endofasica, by ETTORE PATINI. Premiata scuola tipografico dei sordomuti, Napoli, 1907. pp. 42.

This is an experimental study of internal speech, following the same lines as Lemaitre's study "Le Langage interieur des enfants," published in 1904. Patini experimented with twenty-one subjects, giving them, first, as a test of their introspective ability, a short questionnaire on the character of their mental images. He also makes use of Stricker's method of making his subjects try to pronounce mentally some word containing linguals or dentals with the mouth open and the tongue at rest. Patini's subjects were classified under five types, verbo-motor, visual-motor, auditory-motor, verbo-visual and auditory-visual. Lemaitre made a special category of the symbolic-visual, *i.e.*, of those subjects in which a word was represented by its initial letter or some symbolic object, as the word 'continues' by a chain. Patini, however, thinks that such symbolism, which he finds in eleven of his twenty-one subjects, is probably present in all cases of verbal imagination. He makes a distinction between the symbolism which accompanies the ideas of objects and that which accompanies the abstract idea of relation. The present study is concerned only with the for-

mer. The author also notes some individual peculiarities of symbolic representation, such as the representation of words by certain syllables, by initial letters of certain definite sizes, stamped characters with backgrounds of specific size and form. In two of his subjects he found the individual peculiarity, not referable to endophasia, of being unable to represent a straight line. One subject could form no mental image of a rectilinear triangle because the sides appeared persistently curved. Another subject presented the peculiarity of sometimes thinking of the words as written in large characters with accompanying representation of the movements of writing. Patini also observed among his subjects cases of what he calls multiple endophasia, *i. e.*, those who, under one set of circumstances, represented the words in one way and, under another set of circumstances, in a different form, *e. g.*, one subject who was ordinarily visual always became audito-motor when he mentally repeated poetry. This is neither the indifferent nor the mixed type of Charcot, the former of which includes such subjects as are able to regulate the type of their mental images at will, while the latter represent all verbal images in some one of the mixed types, auditory-motor, visual-motor, etc. Another interesting point which Patini notes is the frequent occurrence of a representative pseudo-chromaesthea which corresponds in the representative series to the pseudo-chomanthica of sensation. THEODATE L. SMITH.

Studien zur Hirnpathologie und Psychologie, by A. PICK. S. Karger, Berlin, 1908.

In this monograph Dr. Pick describes a case of "*nachstehender Anamnese*" which is of considerable psychological interest. The examination revealed a pronounced disturbance of memory but no loss of sensibility, or, to quote the author, the "*Sensibilitätsstörung* reduces itself to a greatly impoverished localization." For example, the reaction to the prick of a needle, while following promptly upon the sensation, was so inaccurate as to miss the stimulated spot by 20 cm.; moreover, at times the point of stimulation was not localized at all. Disturbances of orientation were most marked for the head and its parts, but not infrequently for other parts of the body also. 'Asked to indicate the right ear with the finger, the patient may respond readily, but in case of the left ear, stops to think, reaches about the table and only after repeated requests finds the ear.' At times the patient is entirely unable to localize the hands. At other times localization of an organ (the nose for example) is not effected until after the hands have been in contact with it for some time. In such a case the movement is, the author tells us, apparently entirely *automatic.* "If, while her hand is held, the patient is asked to tell where her nose is, she searches for it in the hand that is held, then, tearing her hand loose, grasps it and says, 'Now I did n't *see* it'."

Pick points out that the patient's expressions indicate an absence of visual representation of the parts to be localized. It appears, too, that the so-called automatic movements, taking place before visual imagery was developed, were accurate and apparently normal and *that the visual representations were, in such cases, developed only after the hand came in contact with the parts in question.*

Pick's explanation of these phenomena is that the image of which we normally make use in localization is an optical one; that the first beginnings of the bodily *ego* are compounded of tactile and kinæsthetic sensations, but that gradually these are translated into optical representations until at last the image (of our body) constructed from visual elements has fully taken the place of the tactual kinæsthetic; and that consequently when the visual image is wanting, localization is impossible. E. C. ROWE.

La loi des petits nombres: recherches sur le sens de l'écart probable dans les chances simples à la roulette, au trente-et-quarante, etc, en général dans les phénomenès dependant de causes purement accidentelles, suivies d'une instruction pratique pour le joueur. Par M. CHARLES HENRY. Paris, Laboratoire d'Energétique d'Ernest Solvay, 1908. pp. xiv, 71.

The author of this monograph has set himself the task of discovering a law of small numbers: "est il possible de prévoir, à la roulette, des séquences, si fragmentaires qu'elles soient, au moins de signes d'écarts, les arrivées des événements se conformant finalment d'ailleurs à la loi des grands nombres"? He begins by showing that the appeal to the law of probabilities is hopeless. "Le caractère abstrait des principes et le caractère limité des lois de probabilités impliquent, en même temps que la vanité de tout système qui se réclamerait de la mathématique, la ruine infaillible du joueur"; "le joueur . . . ne peut puiser dans le théorème de Bernoulli, que la certitude éminemment morale de sa ruine inévitable." And if ruin is mathematically inevitable, it is, so to say, rendered more inevitable still by the conditions of the game: the occurrence of the blank, the enormous capital of the bank, and the limit of the maximum stake.

So far, all is plain sailing: Bernoulli's theorem and the law of probabilities. The author now has recourse to principles of 'energetics.' He takes, first, the law of inertia, "en vertu de laquelle tout phénomène énergétique présente une période d'établissement avant son état de régime et une période de persistance après la cessation de l'impulsion." This law may be assumed to apply to the roulette wheel,—if, that is, we may also assume the recurrence of a like starting-point, "d'un état initial périodiquement le même." And the second assumption is justified as a special case of another law of energetics, the law of periodicity, "en vertu de laquelle un très grand nombre de phénomènes physiques et biologiques, après une evolution suivant des lois complexes, mais en principe calculables, se reproduisent au bout de temps plus ou moins longs et qui est la conséquence nécessaire des périodes astronomiques."

There follows the construction of the empirical formula, gained by a conjunction of energetics and psychophysics. Chance, the author declares, is always a subjective phenomenon; "un phénomène fortuit est toujours une sensation remarquable, ou un complexe de sensations remarquable, agréables ou non, exprimable par des nombres entiers qui, la psycho-physique nous l'apprend, representent des accroissements relatifs d'énergie." We thus have the psychophysical formula to start with, and by simplification of terms and acceptance of relevant hypotheses from the sphere of energetics arrive at the empirical formula required. The reasoning in this chapter iv— "Vues théoriques"—is exceedingly condensed, and depends in large measure upon M. Henry's views of general psychophysics, published and unpublished. So far as he understands these views, the present writer finds himself in disagreement with M. Henry upon various points. A discussion would, however, carry us far beyond the limits of the monograph under consideration. It must suffice to say that the author, in his concluding chapters,— "Vérifications expérimentales" and "Importance de l'état initial,"—gives the results of the application of his formula, and is able to show, first, that he gets a good agreement between calculation and observation, and, secondly, that the neglect of the principles of energetics leads to flat disagreement. The work ends with mathematical appendices and with an "instruction pratique pour le joueur."

The author remarks in his preface: "je n'ai pas l'outrecuidance

d'estimer que le joint énergètique et psycho-physique par lequel j'ai cherché à saisir le problème soit le seul possible et je ne me dissimule pas que des développements beaucoup plus étendus seraient nécessaires." The present writer must confess that he finds M. Henry's use of the psychophysical formula arbitrary, and even somewhat fanciful. At the same time, the author appears, in principle, to have solved the problem before him; the empirical formula, as has been said, works well so far as tested, and requires for its working the basis of energetics. Whether the theory can be bettered, and bettered in such a way as to accord still more closely with practice, must remain an open question. In the meanwhile, the author is to be congratulated on an ingenious and successful piece of work. J. E. HAYLEY.

L'Etude Expérimentale de l'Intelligence et de la Volonté, by J. LARGUIER DES BANCELS. L'Année Psy. 13, 1906.

M. Des Bancels calls attention to the new and promising field of systematic introspection, controlled, minute, severe, of which some of the first results are found in Binet's *Etude Expérimentale de l'Intelligence*, 1903, and some very interesting further results in H. J. Watt's *Experimentelle Beiträge zu einer Theorie des Denkens*, *Archiv f. d. ges. Psy.*, IV, 1905, and N. Ach's *Ueber die Willenstätigkeit u. d. Denken*, 1905. These experimenters found that the sensory type varies in the same individual according to the nature of the operations involved, that there is such a thing as a generic image, that a word can be understood before giving rise to any representation, and that a decisive and determining direction is given to the thought by the conditions of the experiment, although this condition or *donnée* shows its presence only by the determinations that it brings about. Thus thought, in proportion as it is direction, organizing force, evades internal observation, and Lachelier is right when he says that "of thought psychologists know only the light that it sheds upon sensation."

CEPHAS GUILLET.

The Wisdom of the Wise; Three Lectures on Free-Trade and Imperialism, by W. CUNNINGHAM. Cambridge, University Press, 1906. pp. 125.

"To try to take opponents at their best seems to be a sound maxim of controversy." Dr. Cunningham, a well known advocate of tariff reform and imperial co-operation, here selects three competent opponents, and discusses the views of Mr. Haldane on economic science, of Mr. Strachey on imperial sentiment, and of Lord Rosebery on the problem of the unemployed. The lectures have all the wonted charm and persuasiveness of the writer's style; their subject-matter is, however, beyond the scope of this *Journal*. An appendix deals with religion and political life, and with the imperialism of Cromwell.

P. E. WINTER.

Laboratory Equipment for Psychological Experiments, by CHARLES HUBBARD JUDD. Vol. III of a series of text-books designed to introduce the student to the methods and principles of scientific psychology. Charles Scribner's Sons, New York, 1907. pp. 257.

The description of the exercises which the author gave in volume II of his series was of set purpose dissociated from description of apparatus in view of the fact that the same laboratory exercises may be tried with a great variety of different kinds of material aids. The presence of this apparatus is better than written descriptions, but the author holds that apparatus is itself a valuable means of education and might well be described as a part of it: but the work is especially designed for graduate students who are preparing to teach or to make

demonstrations. Much of the apparatus is relatively inexpensive and can be supplied by the mechanical departments of Yale and indeed of other laboratories. In general, the order of topics follows some natural psychological definition. Here, optical apparatus comes first, monocular and binocular vision; then come binaural, tonal, and cutaneous sensations, then tactile space, intensities of sensation, modes of recording movement, changes in circulation and consciousness, in muscular tensions, co-ordination, voluntary modifications in movement, analysis of co-ordination, abstraction and fatigue, momentary changes of attention, its scope and that of consciousness, æsthetic appreciation and complex processes.

Association des Idées chez les idiots et les imbéciles, par Dr. Boulenger et Paul Hermant. Gand, Vanderhaeghen, 1906. pp. 137.

A Non Surgical Treatise on Diseases of the Prostate Gland and Adnexa, by G. W. Overall. Rowe Publishing Co., 1906. pp. 228.

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NOTES.

The Sixth International Congress of Psychology.

The sixth Congress of Psychology, in accordance with the action taken at Rome by the last Congress, will meet next year at Geneva. The Committee of Organization chosen for the purpose have fixed upon the time from the 31st of August to the 4th of September, 1909.

The undersigned, desiring that this meeting of the Congress should be as profitable as possible, have under consideration a slight modification of the customary organization. It will be recalled that our previous sessions have drawn an ever increasing attendance, with the result that the communications announced have finally attained an extravagant figure (270 at the Congress at Rome, not counting the twelve conferences of the general sessions). This plethora is not without danger to the existence of a congress. It produces veritable confusion. Since there is literally not time enough for all the speakers listed to present their ideas adequately, the presidents are constantly forced to hurry them and to suppress or shorten the discussions, with the result, too frequently, of a dull dissatisfaction and general *malaise*.

The complaints with reference to the defects of organization in our last congress have been echoed in several of the accounts of its proceedings. We shall cite as an example only an article from the official pen of Professor Ferrari, of Bologna, who in his capacity as secretary of the Congress of Rome was in a better position than any one else to notice the inconvenience of the customary procedure.

M. Ferrari, after pointing out the "decadence" of our great international sessions, expresses himself thus: "The law of life: Alter or perish, applies to the congresses of a science as complex and as incompletely differentiated as psychology. . . . The advantage of international congresses for *savants* and for the progress of science itself is not great. They continue by virtue of laws well known to psychologists; but it is just that knowledge which ought to suggest to psychologists the means of avoiding in the future an agreeable routine, and of profiting in the best way by the time and energy that they are willing to devote to these periodic reunions. . . . The Congress of Rome showed clearly that the necessity of rejuvenating the antiquated and useless organization of international congresses is beginning to be felt. (Bull. Instit. gén. psychol., V, p. 497-8.)

We are sure that the opinion here expressed by M. Ferrari agrees with the feelings of the immense majority of the psychologists who have attended our later congresses.

We seem forced, then, to take new measures in the interest of the institution whose fate for the moment has been placed in our hands. But what reforms shall be offered for this state of things which everybody laments?

Without wishing to settle anything definitely at the present time, we desire to indicate briefly the direction in which we believe that it is necessary to turn in this matter, hoping that this will lead our colleagues in all lands to consider the matter from their points of view and to communicate to us the ideas at which they may arrive with reference to a possibly improved organization of the next congress.

1. Since to-day scientific periodicals are numerous and offer the greatest facilities for publication of all work of value, the true purpose of an international congress should no longer be the reading of innumerable isolated communications upon extremely diverse subjects—necessarily abbreviated and hasty, but should rather be that of allowing a somewhat thorough study and discussion of a limited selection of particularly interesting or vital questions. Our first desire is, then, to place upon the programme of the congress certain *questions of present importance* upon which reports and counter reports should be presented, which latter should also be published beforehand so that those who intend to take part in the Congress may be able to prepare their objections or their communications upon these themes of discussion.

2. We would like in particular to devote some sessions of the Congress of Geneva to the question of *psychological terminology* with regard to which the Congress of Paris, in 1900, expressed the hope that it might be taken up at the next session. Our purpose is to lay before the Congress a plan of terminological equivalents in our chief languages with a view to fixing a certain number of daily more indispensable technical terms having reference to experimentation and perhaps also to certain psychical phenomena or processes. This is, of course, an arduous undertaking and one upon which the coming congress can merely make a beginning.

3. We desire finally to arrange an *exposition of apparatus*, as has already been done at previous congresses. But we would like to have more time reserved for the examination and demonstration of the

apparatus; for this sort of communication can be made only with difficulty and very imperfectly by means of printed memoirs, but is admirably adapted to the function of a congress.

We shall be very grateful to all our colleagues who are willing to do so, if they will send to us as soon as possible their observations upon the points which we have mentioned, suggest to us still other innovations and make proposals as to the choice of subjects to be placed for discussion on the programme of the next congress.

Committee for the Sixth Congress, { TH. FLOURNOY, *President*, P. LADAME, *Vice-President*, ED. CLAPARÈDE, *General Secretary*, Champel, 11, Geneva.

FIFTH ANNUAL MEETING OF EXPERIMENTAL PSYCHOLOGISTS

The fifth annual meeting of Experimental Psychologists was held in the new psychological laboratory of Harvard University in Emerson Hall April 15-17. The following laboratories were represented: Brown (Delabarre), Bryn Mawr (Ferree), Clark (Sanford and Porter), Columbia (Cattell and Thorndike), Cornell (Titchener), Johns Hopkins (Baldwin), McLean Hospital (Wells), Pennsylvania (Urban), Princeton (Warren and Vaughan), Smith (Pierce), University of New York (Lough), Wesleyan (Dodge), Wellesley (Starch), Yale (Angier and Cameron). Communications were presented by Pierce on the Checker Board illusion; by Lough on Some Applications of Tests of the estimation of Distance by the Eye; by Dodge on Types of Pursuit Movements of the Eyes in Cases of Mental Derangement; by Urban on the Psychophysical Methods; by Holt on the Technical Installation of the Harvard Laboratory. An interesting discussion on the place of Laboratory Tests and Demonstration Experiments in Elementary Courses was introduced by Warren. The equipment of the laboratory and the investigations in progress at Harvard were demonstrated and explained by Professors Münsterberg, Holt and Yerkes with the assistance of graduate students in the department; informal reports of current work in other laboratories were also made. The visiting psychologists were hospitably entertained by Professor Münsterberg and his colleagues. It was decided, on the invitation of Professor Warren, to hold the next meeting in the laboratory of Princeton University.

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A QUALITATIVE ANALYSIS OF TICKLING: ITS RELATION TO CUTANEOUS AND ORGANIC SENSATION[1]

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TABLE OF CONTENTS.

[1] From the Psychological Laboratory of Cornell University.

The following study was undertaken in the interests of certain problems in the analysis and classification of organic sensation. It was our belief that the thorough examination of some complex lying, so to say, on the border line between the cutaneous and the organic might open up a pathway to the more detailed investigation of the latter. For such preliminary exploration the use of the phenomenon of ticklishness or tickling seemed obviously appropriate.

While the primary object of experimentation in the work which follows was the analysis of the immediate sensory contents of tickle, the possibility that the uniqueness of the tickle-consciousness may lie merely in the bodily reaction which it excites was not lost sight of. The questions which we set ourselves to answer were as follows. Are the ticklish sensations aroused by a light touch to be classed as cutaneous or organic?[1] Do they represent pressure,[2] a new pain quality,[3] a complex of

[1] By organic is here meant a sensation mediated by non-cutaneous nerve endings, hence presumably unique in quality.

[2] *Cf.* A. Goldscheider, Die specifische Energie der Gefühlsnerven der Haut, 1884; F. Kiesow, *Zeitschrift für Psych. und Phys. der Sinnesorgane*, XXXIII, 429-430; and O. Külpe, Outlines of Psychology, 1895, 148.

[3] S. Alrutz, Undersökingar öfner smärtsinnet, *Upsala Univ. Arskrift*, 1901.

tactual and muscular impressions,[1] circulatory sensations reflexly aroused,[2] or pressure and pain in combination? Lastly, if it is non-organic in origin, upon what conditions or peculiarities of content does the strikingly subjective character of the tickle-consciousness depend?

In the solution of this problem our interest was not confined to the determination of the independence or identity of the points of maximal sensitivity for tickle and those for any other sensation, pain or pressure. The emphasis was rather upon the introspective estimation of the qualitative likeness or unlikeness of tickle and certain simple variations and complications of pain, pressure, and the like. Full experiential knowledge of the cutaneous qualities, under varying intensive, spatial, and temporal conditions, was thus acquired. The methodological value of this material for the further study of organic sensations is readily apparent. For the task of isolating, disintegrating, and classifying such organic complexes as hunger, thirst, fatigue, and the bodily accompaniments of the emotions, certain factors in the introspective outfit of the observer are (as a brief experience proves) indispensable. The first is the possession of a tolerably stable set of standard elements, drawn from the sense departments that are most germane (tactual and kinæsthetic). The second is a rough descriptive catalogue or working knowledge of the apparent modifications of quality[3] which any simple element may undergo, *i. e.*, of the simplest laws of fusion and complication.

The close dependence of progress in organic analysis upon such preliminary knowledge may be illustrated as follows. One of the first questions which arises in the study of organic sensation hinges upon the significance of the distinction between "dull" and "sharp" or "bright." Does the division of organic sensation into two classes, which seems roughly feasible, represent an ultimate sensory distinction? Are there two qualities, the one invariably correlated with plain muscle activity, the other possibly with circulatory changes in the internal tissues? Or are dullness and sharpness modifications

[1] W. Wundt, Human and Animal Psychology; C. S. Sherrington, in Schäfer's Textbook of Physiology, II, 976.

[2] Suggested by M. von Frey: *Berichte d. math.-phys. Classe d. kön. säch. Gesellschaft*, 1894, 192.

[3] The observations recorded in this article, along with a series of introspections on kinæsthetic sensations, tingling, heat and cold pain, neuralgic pain, etc., kept by the writer for a number of months, have resulted in distrust of many verbal distinctions commonly accepted as qualitative and ultimate. The apparent qualitative diversity, reducible to the metamorphoses of a single quality under varied conditions of massing and duplication, is hardly less considerable than the diversity attainable through the varied spatial disposition and intensive modification of a single visual quality, as gray.

of a single original quality, brought about by alterations in spatial extent, intensity, or the effectiveness of attention? Again, the question arises of the affinity on the one hand to pain, on the other to contact, of certain bright or sharp sensations felt internally in the "thrill" of pleasure or excitement, in the tingling of a limb which has been asleep, and in other contexts. In both cases, experimental variation of the conditions in the sphere of cutaneous sensation, and an introspective knowledge of cutaneous pain at low intensities, offer a short cut in the direction of the probabilities which the experimenter cannot afford to neglect. Thirdly, in the study of organic and semi-organic complexes the possibility of the deceptiveness of certain judgments, such as that of localization in the third dimension (*i. e.*, lack of projection beyond the surface of the body), arises. Familiarity with the underlying structural pattern and with the reliability of such judgments in the sphere of cutaneous sensation is the necessary preliminary for any adequate examination of this question.

Section A. Pressure and Pressure Spots

This section of the work was designed as a preliminary to the study of tickling, and was devoted primarily to the determination of von Frey pressure spots and the development of a definite notion of the characteristic response of a single pressure organ to punctiform stimulation. It was our original intention to identify a few responsive pressure spots, to test them for tickle with a series of graduated intensities, to map a given area for points of maximal sensitivity to tickle and pressure respectively, to compare the results, and so to weigh the probabilities in favor of identifying tickle with the modality of pressure. For the carrying out of the first steps in this programme the method of von Frey and Kiesow, of mapping with minimal stimuli, was obviously inappropriate, since the liminal sensations thus utilized are difficult of analysis to the untrained observer, and may well indicate the locality of tickle rather than of pressure organs. We therefore decided to select, for initial exploration, a stimulus intensity capable of evoking the classical 'granular' pressure quality in its greatest perfection and isolation. This method also is open to objection, in the burden which it lays upon introspection; but apart from its immediate value, it promised precisely the training in the discrimination and description of slight tactual differences which was essential to our general purpose.

At the outset of this procedure, however, technical difficulties and introspective complexities began to make their appearance. Since the latter seemed frequently at odds with accepted qualitative distinctions, we felt obliged to postpone the later steps of our investigation while a series of experiments under vary-

ing hypotheses as to the number and affinities of cutaneous qualities was undertaken. When preliminary experimentation with tickle made further apparent the need of a minuter analysis than was anywhere accessible of the sensations resulting from mechanical stimulation of the skin, this line of the investigation was extended beyond our original intention. The modification which our conception of the elementary tactual sensation underwent, in consequence, will be described later.

The majority of the experiments cited in this article were carried out in the Cornell Laboratory in the year 1906-7. Observers were Professor I. M. Bentley, Mr. L. R. Geissler, assistant in psychology, and two advanced students in the department of psychology, Miss M. C. West and Mr. R. Sailor. Professor T. A. Hunter and a junior student, Miss Rosemon, also contributed a number of results. The writer herself occasionally took the part of observer, and a few of the final results are based solely on her introspections, no other observers being accessible at the time.

Preliminary Experimentation

I. Search for the Adequate Stimulus of Goldscheider's Granular Pressure Sensation. In view of the rare occurence of isolated pressure sensations, and the lack of training of most of the observers in pressure problems, recourse was had to Goldscheider's classical description of the pressure quality.[1] Trial was then made in the vicinity of the larger hair-bulbs on the arm or the back of the hand for a stimulus implement which should give rise to the most perfect fac-simile of the sensation thus described.

To our surprise and disappointment, stimulus hairs of medium diameter (such as were used by von Frey and Kiesow) were effective rather in calling out a bright, sharp, superficial sensation, at its higher intensities tending to merge into pain, than in reproducing the compacted deeper-lying sensation, sometimes sharp, sometimes dull, which gradually became the observer's ideal of an isolated pressure. Hairs of small calibre gave only an elusive tickle, bright contact, or a fine stinging pain, immediate or belated. Bristles, glass hairs, and points of carbon, wood, and cork, of various lengths, diameters, and degrees of sharpness were also tested. The blunter gave everywhere practically the same undifferentiated, dull, diffuse sensations, tending to pass imperceptibly into a subcutaneous ache. The sharper and finer, if sufficiently stout, gave now and then a well-defined pressure, too often, however, marred by an accompanying point of pain which, in its weaker intensities, blended subtly with the pressure proper, to the confusion of the

[1] Die specifische Energie der Gefühlsnerven der Haut, Gesammelte Abhandlungen, i, 1898, 77-8.

observer.[1] Wooden points of a certain sharpness and glass hairs with a welded end gave most uniformly results which accorded with the definition.[2] It was desirable, however, for our further purposes to select an implement the intensity of which could be easily graduated. Choice was finally made of a horse-hair of about 0.2 mm. diameter, fitted into an æsthesiometer tube so as to present an adjustable length of 2-6 cm., with corresponding stimulation values ranging from 26 to 2.5 mg/mm (von Frey's tension units), and giving rise in its upper range of intensities to a fair pressure and a minimum of pain. In a few cases a bristle of about 0.33 mm. diameter, 3 cm. long, with a tension value of 36 mg/mm. was employed, the optimal stimulus varying somewhat with the observer and the area investigated.

II. Determination and Verification of Pressure Spots. An area 5 cm. square, situated on the back of the hand or on the volar surface of the arm and near the elbow, was selected for exploration. The locality chosen varied slightly from observer to observer, in every case, however, commending itself by the relative fewness of the hairs and the infrequency of the distracting after-pain or itch which frequently attends stimulation. The hairs on the area chosen (about 2 to 5) were mapped and shaved, and the area explored bit by bit; the observer, who by this time had acquired considerable experience with punctiform cutaneous stimuli, indicated the points at which the resulting sensation squared with his pre-established notion of pressure. The effort was made to abstract as far as possible from the influence of mere sharpness or pain in the formation of these judgments, and the observer employed as criterion the memory-images of pressure developed in the course of previous experimentation.

The distinctive features of this sensation, while practically identical for the different observers, were variously phrased, as "not sharp, but well-defined or well massed;" "a solid core of pressure under the skin;" "muscular feel, deep, lumped-up." The points chosen usually lay to the windward of a hair, as remarked by von Frey.

[1] These two facts (apparently overlooked or minimized by most observers), the existence of bright but fairly painless pain sensations, and the overlapping of pain and pressure stimulus ranges, reveal the untrustworthiness of the customary laboratory direction to map for pressure by means of the clearest and most distinct sensations. The presence of a weak but hardly distinguishable pain must frequently exercise an influence on such judgments.

[2] Later results, harmonizing even more exactly with the definition, were obtained by the device of a quickened thrust of the stimulus hair, or by the application of a pin-point (over the more insensitive pressure bulbs). This latter method seems, on careful reading, to be the approved one for the production of the typical pressure sensation in the writings both of Goldscheider and of von Frey.

Evidence of the stability of the points thus determined was very halting. In spite of the greatest care in marking the skin, and in guarding against errors due to slipping, the point of attack yielding the best granular pressure on one day had often shifted considerably on the next. Repeated stimulation of the point during the same observation period, for the purposes of verification, often resulted in the substitution of a piercing pain for pressure. Finally, in re-examining the area with weakened stimulus intensities (at the expense, of course, of the genuine "granular pressure" quality), it was frequently noted either that the points of greatest sensitivity under these altered conditions did not coincide with the marked pressure spots, or that points of equally vivid sensibility were discoverable in the intermediate areas. The quality of these intermediate points, under the conditions, was bright but not painful.

III. Electrical Stimulation of Pressure Spots. Weak faradisation of the region above the hair follicle was then resorted to, as possibly more efficient than mechanical stimulation for the exact determination of the position of the pressure spot. To our surprise, the first sensations to emerge with increase of current were not those of pain, as the observations of von Frey had led us to expect, but certain shadowy, quivery shreds of sensation, which with increase of the current became a sharp tremolo or tingle, totally foreign to the specific pressure quality adopted as the standard in our observations. With continued increase of current, these pricking, tingling sensations seemed to fuse into a sharp tearing pain, which, however, rapidly died away. We sought in vain for anything corresponding in our estimation to the throbbing pressure sensation reported by von Frey as the characteristic occurrence. The only suggestion of a pressure response reported by the observer was an occasional dull areal pressure of considerable extent, usually referred by the observer to the contraction of the muscle fibres of the skin, and not intermittent but steady, lasting even after the removal of the electrode; or a very deep dragging pressure, seemingly below the pain zone and hardly distinguishable from ache. The term 'pressure' was here, however, obviously applied in its popular objective sense, not at all in the restricted technical significance given it in this article. A similar criticism applies to the throbbing pressure which was finally obtained from the gums. It was obvious, then, either that electricity could not be employed in mapping, or that we had somehow gone astray in our selection of the typical pressure sensation.

IV. Comparison of Pressure Sensations from Different Areas. Examination was then made of isolated pressure sensations from different regions, in order to discover whether the sense

contents which we had adopted was really typical or was merely an artifact, dependent on the composition of the tissues, etc., in the regions stimulated. Pressure sensations from the palm, the gums, the lips, and the hair-bulbs of the calf of the leg were examined and compared with those from the arm.

Striking differences immediately became manifest. From neither the palms, the gums, nor the lips could a truly granular, "lumped-up," "compacted" quality be obtained. From the mucous membrane of the gums only a bright sensation, verging on pain, and fading quickly into a dull diffuse after-sensation, with nothing of the true granular or knotty character about it, was obtainable. From the palm (where prolonged observation is greatly hindered by the secondary pain or itch sensation accumulating as the result of stimulation for pressure) a 'thrilly,' 'fizzly,' 'boiling' or 'swarming' feeling was the prominent feature. With a slightly different set of the attention, however,—probably on an infinitesimally later phase of the sensation,—a duller, deeper *pressure* would be remarked, and the "thrilly" aspect sensed only as a bright fringe or halo, half ticklish. Rarely, an intermediate stage was caught, when the thrill and the duller core seemed to fuse into something nearer the granular pressure of the back of the hand. This strengthened an interpretation which had already suggested itself, that the so-called 'granular pressure' is a complex, consisting possibly in a fusion of one bright tactual sensation with a duller component, excited by the impact of the hair-bulb or touch corpuscle on deeper-lying nerve-endings.

In case of the lips, also, the sensation is 'tingly' rather than granular and compact. From the pressure spots over the hair bulbs on the calf, which were tested with a sharp pin-point (Goldscheider's method), results intermediate between those from the palm and those from the arm were obtained. The sensation possessed body, tridimensionality, a dullish substrate, but the sharpness aspect, which too often verged on the painful in the case of the arm area, was here milder and in some cases apparently complex, as if a tiny group of bright points were incompletely fused with a certain dull component into something closely approximating the typical granular pressure.

V. Serial Stimulation of a Pressure Spot. In order to examine the sensation differences correlated with different stimulus intensities, and the possible appearance of tickle at the lower limit of the scale, the marked spots over the hair bulbs were stimulated with a series of intensities ranging from 2.5 to 26 mg/mm. Four different series, varying in length from 4 to 15 steps, were given, ascending and descending alternately. At each step in the series the observer was required to give a qualitative judgment.

In the results of all four observers, four apparently qualitative differences made their appearance in succession in the series.[1] Each of these — tickle, contact or brightness, pressure and pain — was correlated with a fairly well defined range of intensities in the stimulus scale. These ranges were, however, somewhat overlapping, and indeed the qualities themselves sometimes appeared in conjunction. Further, the sequence of the contact, pain, and pressure qualities varied somewhat according to the length and direction of the series, a fact apparently correlated with the operation of fatigue.[2] The intensity ranges corresponding with the four qualitative phases were as follows: tickle, 2.5 to 8.5 mg/mm; contact, 5 to 11 (rarely to 22); pressure 8.5 to 26; pain, 25 to 26.

Typical series for three of the observers run as follows:

B.	26 mg/mm.	Pressure, sometimes ending in a painful prick.
	20	Pressure with brightness in it.
	13	Lively pressure.
	8.5	Brightness core.
	5	Diffuse.
	2.5	Tickle.
G.	26	Pressure.
	20	Well-defined sharp contact with pressure below.
	13	As before.
	8.5	Well-defined sharp contact.
	5	Tickle or contact, pointed.
	2.5	Doubtful. Creeping tickle?
S.	26	Pressure and pain later.
	20	Pressure and contact.

[1] The observers were especially cautioned to base their judgments on the immediate sense-experience, and not on subsequent inference or objective reference. The translation of tactual sensation into terms of the stimulus is, however, so ingrained and instantaneous that purely qualitative analysis is greatly impeded. Immediate visualization of the stimulus or of the denting of the skin was the rule; sometimes an interpretation in kinæsthetic or verbal terms was substituted. It is perfectly possible, then, that the descriptive distinctions maintained represent neither disparate sensation qualities mediated by different endings, nor diverse phases within a single qualitative continuum, but verbal associations merely, conditioned primarily by different degrees of intensity and diverse *Gestaltqualitäten*, secondarily by the visual sharpness or bluntness of the imaged stimulus. The intensity explanation seems, however, negligible. Introspection goes to show that, with weak mechanical stimulation, qualitative differentiation of contents is prior to the perception of intensity differences. On the other hand, the dependence of these descriptive distinctions upon unanalyzed complexities of content—due *e. g.*, to the spread of stimulation to adjacent endings, to slight temporal or intensive irregularities, and the like—is a question which demands attention with reference especially to granular pressure and tickle.

[2] Thus contact or sharpness may appear in a descending series only in connection with the first (most intensive) stimulus, instead of extending over its customary range of intensities.

13	Contact.
8.5	Contact.
5	Contact, bright, punctiform, definite.
2.5	Tickle or contact.

The terms in which the different observers phrased their differentiation of the four qualities were as follows. The isolated pressure quality, as obtained from the arm or the back of the hand, is deep, compact, definite, massive or "massed," dull or muscular, easily passing into an ache. "Contact,"[1] on the other hand, is superficial, sometimes punctiform, sometimes areal, bright or sharp, shading into tickle at the one end of the intensity scale, into fine sharp pain at the other. More extrinsic points of opposition are found in the temporal aspects of the two. Contact rises abruptly and fades quickly, while pressure is more lasting, dying away slowly into a long dull after-image. Again, as to the degree of projection, of objective reference or localization in the two cases: contact is referred to the surface, with visualization either of the skin area or of the stimulus; pressure, on the other hand, is not projected to the surface, is usually sensed "below the skin" with less precise visualization and localization, but with a certain spatial character of its own, apparently conditioned in most cases by a very vague visualization. Pressure is thus conceived of as having weight, body, bulk, a "lumped-up" or "tridimensional" character, as over against the bidimensional nature of contact or brightness.[2]

A word must be said further on the use of the term 'dull,' and on the possibility that the so-called granular pressure sensation represents not the elementary tactual quality but a complex. Three of the observers showed a tendency to fall back upon the popular use of the term pressure; they associated it with the dull massive sensation correlated with the impinging of any extended solid object on a fleshy portion of the body. Granular pressure, it was discovered, meant in many cases a definite, rounded-off portion of this pressure, welded more or less perceptibly with brightness (contact). A further evidence of the significance attached to the attribute of dullness appears in the statement that pressure "feels as if the skin were dead;" in the greater ease of recognition of pressure spots, when the bright superficial quality had been "fatigued out," as the observer put it; and in the liability to confuse the

[1] More exactly the sensation correlated with moderate intensities; called 'contact' by two of the observers only.

[2] The question as to how far these criteria represent associative factors, correlated merely with degrees of intensity or diffusion of stimulation, will be met later.

light diffuse surface sensation from intermediate areas with weak stimulation of an over-worked pressure organ.—

A brief description of tickle and its relation to contact may here be inserted, though a fuller account will be given in the section on tickle proper. The sensations arising at the lower limit of the stimulus scale were fugitive, vague, elusive as color sensation on the periphery of the retina, and were classed by the observer as 'doubtful,' 'faint tickle' or 'contact'. The distinction between tickle and contact at this level is, in the opinion of the observer, one of objective reference and definiteness of localization rather than of quality pure and simple. Two forms of tickle are distinguishable, in the case of punctiform stimulation; neither bore any obvious resemblance to granular pressure. The first occurs on the palms or lips; it is quick, sharp, lively, akin to pain, thrill-like or vibratory, fairly well localized, but with considerable irradiation and after-image. The second, occurring in the vicinity of hair-bulbs on the arm, hand, etc., is faint (but hardly dull), fine (but hardly distinct), on the surface or just below (but very hazily localized), brief (but usually long enough to display a faint flickering).

Summary of Results

The significant points in these initial experiments may be summarized under four heads:

1. The difficulty of securing an isolated granular pressure without pain.
2. The difficulty in the precise localization and verification of pressure spots; and the apparent existence in the intermediate areas of other spots of approximately equal sensitivity.
3. The inability to reproduce the granular pressure sensation by electrical stimulation of verified spots.
4. The differentiation of pressure and contact (or sharpness) in the introspective results, and the similarity between the latter (rather than the former) quality and tickle.

The theoretical bearing of these facts, and their congruity with histological and physiological data must now be examined.

Theoretical: The Histological Substrate

1. *Original Assumptions.* Our procedure as originally planned was based on the tacit acceptance of certain points in the von Frey doctrine of cutaneous elements.[1] First, the sensitivity of the skin to pressure is, strictly speaking, punctiform. Each point of greatest sensitivity (*Tastpunkt*) corresponds to a

[1] As set forth in his "Untersuchungen ü. d. Sinnesfunctionen d. menschlichen Haut," *Abhandlungen d. kgl. sächs. Gesellschaft*, 40, 1897. The results published earlier, in the *Berichte*, differ considerably in certain important details.

single buried end-organ, either a Meissner corpuscle or a hair follicle with its wreath of nerve endings. The cases in which, on hairy areas, the number of pressure spots exceeds the number of hair bulbs is negligible. The characteristic sensation resulting from the isolated stimulation of a pressure organ is the granular pressure of Goldscheider. Pressure and contact (*Berührung*) are merely different phases of the same quality, the latter fainter and more fleeting. The characteristic pressure sensation may be produced by electrical stimulation of the pressure point, with direct or induced current. Lastly, the sense departments of pressure and of pain are disparate, each possessing its own nerve endings and its own intensive limens.

The task of harmonizing the introspective intricacies of our own observations with this position was an increasingly perplexing one. The most persistent points of discrepancy in our results were as follows. First, pressure and contact (or brightness), as produced by different stimulus strengths on the same spot, differ in quality rather than in intensity proper. Secondly, contact or brightness spots occur in the areas between the pressure spots, and yield sensations which differ from those of the pressure spot (at the same stimulus intensity) in body and complexity, not in definiteness and clearness. Thirdly, the electrical stimulation of pressure spots evokes a kaleidoscopic group of sensations which are individually more akin to the contact or brightness quality than to granular pressure. The appearance of pain in connection with pressure spot stimulation is not in itself incongruous with the von Frey separation of pain and pressure, since good evidence has been brought of the double innervation of the hair follicle; but the existence of a range of weak pain intensities, accompanying pressure spot stimulations and closely imitating contact or brightness sensations, is a fact demanding closer scrutiny.

It seemed probable that differences in method and outlook, in the strictness with which qualitative criteria were applied, lay at the root of these discrepancies. For example, the granular sensation produced by mechanical stimulation of the hair-bulb undoubtedly represents an unique sensory contents. Suppose that, without inquiry into its simplicity, this sense-contents is accepted as the standard, that the limiting value for noticeable stimulation of such points is determined, and that the pressure spots of any given area are mapped upon this basis. Then, even if outlying points existed which, along with an equally vivid sensibility, possessed slightly higher limens, it is obvious that by reducing the trial stimulus-intensity these might be eliminated, and the number be narrowed down to one to a hair-bulb. Further, since the granular pressure sensation belongs uniquely to the spots thus selected, their less distinctive

phases of sensation might easily be overlooked or assimilated to the "granular" contents; and the unlikeness of the equally unique response of the spot to electrical stimulation, together with the existence of the intermediate points of sensitivity, might be dismissed as irrelevant anomalies. The procedure thus outlined is, on the whole, the most straightforward and consistent from the point of view of the physiologist who is in search of definite points of pressure sensitivity. In the absence, however, of corroboratory histological evidence, we are free to consider whether some other interpretation of the facts may not do greater justice to the psychology of the question. The difficulty of reconciling our observations on tickle with the doctrine of granular pressure gave us an added motive toward some revision of our original hypothesis.

2. *Tentative.* At this point of our inquiry the facts seemed to indicate some such schema of the qualitative categories as the following:

a. Pressure and 'contact-brightness' or touch are disparate qualities. Blunt and sharp represent primarily qualitative rather than perceptual differences. The dullness which characterizes the former is not the product of the welding together of a number of punctiform sensations at low intensities, but is an unique sensation quality. It finds a familiar representative in the 'pressure' of everyday speech, the sensation (*minus* its kinæsthetic accompaniments) of a heavy body resting against the skin. The contact quality, on the other hand, is more akin to tickle, and is above all bright, vivid, definite.[1]

b. Contact-brightness and pressure are mediated by different histological elements, the former by fibres ending around the hair follicle and in the more superficial layers of the skin, and very susceptible to fatigue; the latter arising from deeper-lying elements, free endings in the vessels, Pacini corpuscles, muscle spindles or the like. The greater efficacy of pressure over the hair-bulb in the production of the latter sensation is probably due to the concentration of the mechanical effect through the grinding down of the comparatively solid follicle upon the underlying endings. The more superficial set of endings furnishes the sensory data for precise localization and cognizance of the finer detail of tactual impressions, angularity,

[1]The distinction here formulated seems closely related to Meissner's differentiation of touch from pressure (*cf.* his *Beiträge zur Anatomie und Physiologie der Haut, 1853*); a distinction which it has been customary to dismiss as a confusion of cutaneous and kinæsthetic factors, the passive and the active elements of touch. A similar distinction has been made by Bronson in his discussion of tickle and the persistence in man of a primitive contact-sense mediated by the epidermal fibrillae (*cf. The Medical Record*, XXVIII, 425).

figure, etc., on the analogy of the now discredited "Ortsinn" of the earlier physiologists and psychologists. The other set of endings may be taken to represent the sense which, in conjunction with ache or pain, figures in the passive estimation of differences of weight.[1] Evidence of the histological independence of these two senses may be found in the observations of Head.[2]

c. The so-called granular pressure is not itself an element, but is a complex of deeper pressure and contact as above defined.[3] Hence arises the difficulty of exact reproduction by superficial faradisation of the skin. Hence also comes the disagreement of investigators as to the occurrence of pressure on certain areas where the appropriate conditions for the coupling of these two components of granular pressure are absent, *e. g.*, the cornea.

d. Lastly, it is possible that "contact-brightness" and pain represent not distinct qualities, but shades, so to speak, within the same continuum. The fact that pain arises from intensive electrical stimulation of the hair bulbs might be considered a point in favor of this hypothesis; but the occurrence, under various forms of stimulation, of ambiguous stabbing sensations which introspectively are equally akin to contact and pain furnishes a stronger argument.

The possibility of harmonizing the findings of von Frey with this position is perhaps less remote than might at first glance be imagined. It is not unlikely that von Frey in his investigation of *Druck* or *Tast* has usually in mind the bright, quick quality which we have differentiated from pressure, and that the granular pressure of Goldscheider has significance in his

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[1]Weber's law has been demonstrated only for comparatively high stimulus intensities (100-300 grammes and above). The lower intensities (100 gr. and below) which, according to our hypothesis, would be mediated by the superficial contact endings require different stimulus progressions, and the sensations themselves are subject to a rate of fatigue too rapid for their satisfactory use in difference determinations. *Cf.* H. Griffing, On Sensations of Pressure and Impact, *Psych. Rev. Monograph Supplements*, I, 1895, 30 and 42.

[2]*Cf.* his discussion of the disassociation of deep and superficial pressure following injury to the radial and external cutaneous nerves, *Brain*, 1905, Part 2, 99. While stimulation with cotton wool, or a camel's hair brush is unappreciated, a light touch with the point of a pencil, the head of a pin, or the tip of the finger is immediately sensed.

[3]The difficulty of isolating cutaneous sensation from that of the underlying tissues is commonly conceded. The question at issue here is somewhat different, inasmuch as it concerns the complexity, not of the whole sense-contents attending mechanical stimulation, but of the granular sensation in itself. At this point of our analysis, we were inclined to regard the deeper component as the unique factor differentiating granular pressure from the sensation of intermediate points.

system rather as an unique pressure-spot experience than as the ultimate qualitative unit. His classification of paræsthesias, tingling, and electrically excited sensations with pressure or *Tastempfindungen* seems, indeed, to point to this conclusion. Again, with regard to the differentiation of dull and sharp, von Frey himself in a recent statement[1] has recognized the distinction between touch and deep pressure as valid, conceding the conclusiveness of Head's evidence, and suggesting the muscle spindles, or the Pacini or Golgi-Mazzoni corpuscles as the terminal organs for the latter quality. Lastly, as to the number and distribution of touch points and the status of the intermediate points yielding only bright (not granular) sensations, the original *Berichte* reports would seem to agree fairly well with our position. The reduction of the number of spots to the hair, in the *Abhandlungen* and in Kiesow's later work, is the result of a more or less arbitrary lowering of the test intensity (from 4 to 0.5 mg/mm) in different cases.

So far, then, our proposed revision has found no essential contradiction in the observations of von Frey. It remained only to submit the schema to the test of experimentation. The modification which the hypothesis underwent in consequence will be discussed later.

Further Investigations and Résumé of Results.

If pressure and touch actually represent not different intensity phases of the same quality but discrete modalities, experimentation should be able to show that the two vary independently, are affected differently by fatigue and drugs, possess different limens, and probably differential features in the conditions of their adequate stimulation in general. Even if their terminal organs are frequently associated, some cases ought to be found in which the points of greatest sensitivity are not identical; and, to eliminate the explanation of the quality differences in terms of intensity, the two should be shown to occur not only in isolation with the same stimulus intensity, but also simultaneously at the same point of stimulation. The following series of test experiments were accordingly undertaken.

VI. Mapping the Pain, Pressure, and Contact Points about the Hairs: Testing with Serial Stimulation. For the first purpose the æsthesiometer, at an uniformly high intensity, was employed. The results went to show that the points of greatest sensitivity for the different qualities are usually distinct When a series of graded intensities is applied to the different spots, contact-brightness and pain sensations usually emerge

[1] *Cf. Jour. Amer. Med. Assoc.*, XLVII, No. 9, 1906, 647.

at the appropriate stage in every case, but on the pain or contact spots a dubious pressure or none is reported.

VII. Mapping for Pressure, Contact, and Tickle with Stimulus Intensities of 25, 15 and 10 mg/mm. A new hair, 0.23 mm. in diameter, was used. It gave in general its best pressure, contact, and tickle at the above intensities. Mapping was facilitated by the discovery that a quick short stimulation gave the best contact, while a more leisurely setting down and removal of the hair gave the more definite pressure. The tickle spots were found to coincide with the pressure rather than with the contact spots. Lifted folds of the skin and the region over the knuckles were also tested for pressure, which was easily obtained from the latter, but was apparently lacking in the case of the former.

VIII. The Occurrence of Pressure and Contact in Conjunction in Certain Cases, in Disassociation in Others. In the first case, prolonged stimulation with a blunted point, in the other the application of a series in the manner above described, was the procedure. In a large number of cases the observer reported the appearance of the two qualities together; sometimes the one, sometimes the other came out in advance, in accordance, apparently, with the direction of the attention. In a certain number of cases the contact reported later was probably a weak phase of a secondary pain. In the disassociation experiments, in which the area examined was first fatigued to pressure or rubbed with cocaine, the contact or brightness quality always suffered, while deep pressure was not only not at all weakened but apparently came out the more clearly in the absence of the superficial brightness.

So far, the results seemed sufficiently in harmony with our hypothesis. But the query now began to press as to the significance of what we have called our brightness or contact spots, occurring between the hair bulbs. Are these on precisely the same footing as the spots over the hair bulbs? Do they not possibly represent the free epidermal endings, *i. e.*, the pain spots of von Frey,— the contact or brightness quality being merely a fainter intensity of that sensation? The accidental discovery that superficial pain adapts out almost as readily as superficial contact gave further incentive toward the examination of this phase of the problem. The concluding experiments, which brought us ultimately to a more satisfactory conception of the quality of pressure, were devised more especially in the interests of this question.

IX. Mapping for Pain and Testing for Contact-Brightness. Precautions were taken to ensure the discovery of all the super-

ficial pain points on a given area of the arm; the skin was softened and a finely pointed needle was used. It seemed to us quite likely that so-called cutaneous pain is mediated by two sets of endings: the intense but quickly-fading variety by the epidermal nerve endings, the more severe and lasting (including ache and possibly secondary pain) by deeper (possibly vasomotor) endings. Our brightness sensation, if related to either, was most related to the former set. Hence in mapping we were careful to permit the point of the needle to penetrate only the superficial epidermal layers. After a due period of rest, the points so mapped were examined with the hair æsthesiometer (at about 23 mg/mm.), and were found to give in many cases the typical brightness (contact) sensation; in others, contact followed by a point of pain; or thirdly, where the epidermis was evidently injured or the region beneath had reddened, a pain which tended to outlast stimulation.

Later, in retesting for pain, the responsive point was often found to have shifted slightly, in a way corroborating Thunberg's hypothesis of an areal pain sensitivity corresponding to a tiny cluster of brush endings. The shifting would then represent the successive fatigue of endings within the same brush.[1] Mapping of the same area for pain with an overheated brass point, lightly applied, gave practically identical results as regards the position of the points and the occurrence of fatigue. That superficial heat pain is mediated by fatiguable nerve-endings is, of course, indicated by the rapid decrease of pain intensity on plunging the hand into a bowl of hot water. That this represents peripheral fatigue, not a mere loss of conductivity in nerve fibres in general, is indicated by the retention of other sensations, especially certain vivid (but unpainful) superficial sensations of pricking or tingling.

X. *Mapping for Contact-Brightness and Testing for Pain and Pressure.* An experiment complementary to IX. was then performed on a fresh area. The hair æsthesiometer at a length representing about 16 mg/mm was used; but in order to avoid a secondary pain response it was set down gently, and probably its effective intensity was somewhat lower. The points giving a clear brightness sensation were mapped, about 16 for an area of 3 by 10 millimeters, or approximately 50 to the square centimeter. This number is rather too large for the pressure spots of the area (on the volar side of the arm), and too small for the

[1] The possibility of fatigue in undifferentiated endings runs counter to current dogma, but there is (so far as the writer knows) no histological or physiological evidence of its impossibility in the case of the free epidermal endings; especially if, as seems probable, mechanical excitation itself is effective only through the production of chemical alteration in the nerve tissue.

pain spots, according to the calculations of von Frey. The area was next tested with a wooden point; some three or four of the marked points gave, not a single sharp sensation, but an obviously complex multiple of such sensations which was recognized as granular pressure; the others gave merely sharpness or a vague diffuse sensation. The hair æsthesiometer at 24 mg/mm intensity gave approximately the same results, the single points, however, becoming almost painful. The needle used in IX. was then applied gently to each point. Nine of the marked spots gave a lively pain immediately, sometimes, however, displaying an infinitesimal shifting of the point of greatest sensitivity. The rest gave granular pressure or nothing, while as many new pain points (about 16) again came to light in the gaps.

So far, then, it seemed clear that the bright component of granular pressure is fairly indistinguishable from the sensation arising from a weak stimulation of a superficial pain point; and, further, that the existence of contact points independent of pain and pressure spots is untenable. The question of the varying complexity of the sensations arising from hair-bulb stimulation, in comparison with the uniformly simple sensations arising from intermediate points, is one that requires further examination.

XI. Mapping with Electricity for Pain and Pressure. The interrupted current was used to explore an area on the lower arm. The indifferent electrode was bound to the arm with a damp sponge; the stimulating electrode was a fine point of copper wire. With gradual increase of the current, the observer reported the first appearance of sensation directly over the hair bulb as a faint "whirr" or cobwebby flutter or flicker, at times almost ticklish. Sometimes a brief stab, single or barely oscillatory, could be obtained from the intermediate regions with the same intensity of current. With approach of the primary and secondary coils the vague "whirr" over the hair bulbs became a "buzz," a complex tremolo of sharp, almost painful, points. Two such points of equal sensitivity were usually found in the vicinity of the same hair bulb. A similar response occurs in the intermediate regions only when the electrode is applied over a vein or artery, or when the current is strong enough to diffuse to neighboring hair bulbs. The sensation evoked in the interstitial spaces is usually sensed as a sharp, quick pain, immediately disappearing or fading into a weak, sharp tremolo or trill, quite different from the complex seething of hair-bulb stimulation. The pain itself, if not too sharp, may be perceived to possess an oscillatory rather than a steady character.

If the current is again increased, and fatigue of the area is avoided, the sharp "buzz" over the hair bulb seems to fuse into a tearing, rending pain, uneven, not perfectly continuous, and rapidly fading into a series of wrenching pricks which (while actually painless) excite a strong and almost reflex tendency to withdraw the arm. The pain thus evoked from the hair bulbs is strikingly different from the pain of the single intermediate points, which is fine and wirelike, while this is less abruptly penetrating but indescribably 'wrenching.' A similar distinction was made by one of the observers with regard to the pain produced by bristle stimulation of the hair-bulb and the intermediate regions. The former was characterized as sparklike, branching, explosive, aching, or dragging, the latter as wirelike, piercing. This difference is obviously less one of quality than of complexity, and the possibility is suggested that just as the granular sensation represents a synthesis of sharp points, so the hair-bulb pain represents the fusion of a number of single pain sensations.

In the course of this experiment, the discovery was made that a brief down-setting and removal of the electrode (with a moderate current) produced a fair imitation of the granular pressure of certain regions. This gave rise to the suggestion that the hitherto puzzling electric sensations from the hair-bulb represent merely a disintegration of the sharp component of the granular pressure, *minus* its duller, deeper substrate; and, further, that this sharp component represents the essential factor in granular pressure, the idea of compactness or solidity being derived from the manifolding and fusion of these components in the case of mechanical stimulation.

With the practice in analysis gained through these observations of electrically excited sensations in different complications, at different rates and at different points of fusion, the pressure sensations arising from mechanical stimulation on different areas were again passed in review, and a final estimate of the true pressure quality was attempted.

XII. New Introspections on Granular Pressure. From the cornea and conjunctiva no pressure, granular or diffuse, was obtained. Hairs of different sizes and loops of silk thread were used for the test. Sharp, punctiform, painful sensations uniformly resulted. The conjunctiva was less acutely sensitive, but the aching, areal sensation sometimes there evoked could never be identified as a dull pressure. The only tickle obtainable was painful.

On the mucous membrane of the mouth a good granular sensation could be obtained only by thrusting in the point of a needle. From the tongue, the hair æsthesiometer called out

only a sharp sensation, but by fusion with an aftermath of apparently reflexly excited sensations in the neighborhood this yielded a fair imitation of the granular sensation.

The observation that the "crunching" characteristic of granular pressure was best called out by stimulation over a bony substructure or on a calloused surface was now supplemented by the accidental discovery that on reddened or swollen areas the light, quick application of a needle point or hair gives, between the hair-bulbs (especially if the area is already fatigued to pain), a complex of unpainful, sharp sensations equivalent to a granular pressure *minus* the dull, deep component. If this sharp, thrill-like aspect of the pressure sensation were taken as the essential component of cutaneous pressure, the difficulty of classifying tickle as well as the electrically excited sensations with the cutaneous sense would lapse; the three would represent merely different spatial, temporal and intensive arrangements of the same elementary brightness sensation.

General Conclusions

1. *The Elementary Tactual Quality and the Histological and Physiological Conditions of Its Occurrence.* Three points out of the above observations are of special significance to our purpose.

(*a*) The complexity of the "brightness" component in granular pressure.

(*b*) The similarity of weak pain to any one of these bright components taken singly.

(*c*) The probability that these brightness components represent the essential, *i. e.*, the typical feature in granular pressure, and that the dull substrate which accompanies it under certain conditions is a secondary and probably a non-cutaneous factor.

Weighing these considerations, in common with the introspective evidence of Section B., we suggest that a single brightness or contact impression be considered the tactual unit. This qualitative unit, appearing in various groupings and intensities, may be sensed as granular pressure, contact, tickle, possibly even as pain. The bright, quick thrill of a light touch, and the stinging but unpainful sensations from twisting massage, may be considered as familiar representatives of this quality.

Physiological and histological conditions probably determine the variations in the form or pattern in which the tactual quality appears. The histological substrate of the sensations of light pressure is probably the Meissner corpuscle or the nerve endings of the hair follicle. The feature in common in the two

anatomical structures is the concentration of the terminations of one or more nerve fibres within a small space, accessible to stimulation from a limited skin area only. In the Meissner's corpuscle branches representing two, three or even more neurones (in the case of the larger corpuscles) break up into a complex network of varicose branchlets within a connective tissue capsule. In the case of the hair, only one nerve fibre as a rule reaches each follicle, at about half the distance down the root from the surface of the skin. This divides into two branches, from which numerous varicose fibrils proceed upward for a short distance parallel to the axis of the follicle, terminating apparently outside the glassy layer, that is, outside the solid structure of the hair root.[1]

Now, whatever may be the mechanical or molecular changes within the surrounding tissues which condition an excitation of these endings, it seems likely that the endings within a group are capable of individual response, that they may be excited singly, in irregular succession, in varying patterns and in different intensities, according as stimulation reaches them in the form of a single prick from the point of a needle, a light thrust from a blunter object, or the passage of an electric current. Possibly the chances of irregular stimulation are increased by some tendency toward periodic alteration in the adjoining tissues, *e. g.* through the vibratory movements of the hair or hair root. It is also possibie that the endings are accessible to stimulation from more than one point on the overlying skin,—a possibility confirmed by our discovery of two to three points of almost equal sensitivity in the immediate vicinity of the hair follicle.

That the sensation resulting from the stimulation of such an ending singly should be identical with the quality resulting from the excitation of a free epidermal ending contains nothing *a priori* improbable. Histological evidence of the differentiation of the terminal fibres of either set of nerves is lacking; and, further, it has been shown that the hair follicles in general receive their nerve supply from the same fibres as terminate in the immediate skin area. While differentiation of quality is sometimes assumed to be centrally rather than peripherally conditioned, it would seem probable that the original motive to such differentiation sprang from some peripheral stimulus difference. Now the adequate stimulus of both pressure and pain is mechanical, differing only in area or penetrativeness and in intensity. While this difference is of the highest biological significance (since highly intensive stimuli are almost uniformly injurious to the tissues), it seems possible that it is amply represented in the interests of biological functioning and of protective reflexes by the functional and anatomical independence of the two sets of nerves, for pressure and pain proper. In the one case (that of the epidermal endings) we have great accessibility to such external stimuli as would tend to pierce or

[1] A. A. Böhm, M. von Davidoff, and G. C. Huber: Textbook of Histology, 1904, 393-4.

penetrate the skin; in the latter (hair wreath or Meissner corpuscle) we find an arrangement whereby the endings are both protected from excessive stimulation and fatigue, and fitted for the perception of the faintest impressions, through the multiplication of the number of endings distributed within a minute space or by a special device (the hair) for magnifying the mechanical effect.[1] This anatomical and functional separation of the two sets of nerves insures, of course, distinct reflex arc connections for the two grades of stimuli.

From the point of view of consciousness, the distinction between blunt and sharp (*i. e.* the areal difference in stimuli) is provided for by the complexity or simplicity of the resulting sensory process in the two cases, the sharp response (from pain or pressure organs) being possible only with sharp and penetrating or pointed objects, the granular or pressure sensation arising ordinarily from objects presenting some surface. The distinction between extremes of intensity may possibly be represented by a qualitative distinction dependent not on differentiated endings but on independent central stations for collaterally and directly conducted impulses; the collateral connection would then lie open only to high stimulus intensities. In any case, the motive for qualitative differentiation of pain and pressure within their lower range of stimulus intensities is not conspicuous, and this is the point with which we are here concerned. Upon the actual fact of the likeness of the simple tactual quality and the distinct but painless response of a pain spot to moderate stimulation, we believe the testimony of introspection to be sufficient.[2] The readiness with which the latter sensations pass over into positive pain has doubtless contributed to screen them from comment.

To return to the pressure-unit concept, and to recapitulate: the granular pressure of Goldscheider was, as our introspections showed, a complex sense-experience. The desirability of retaining within the category of pressure such semi-disintegrations as tickle and electrically excited sensations forced us toward recognition of this complexity and revision of our pressure-concept. In the accomplishment of the latter task we have discarded the dull substrate of the granular pressure of our earlier observations as an inessential concomitant of cutaneous pressure, having its origin probably in the subcutaneous tissues.[3] Abstracting from this deeper component, we find the isolated skin pressure still complex or "granular," made up apparently of a number of simple tactual impressions whose description is impossible except by such adjectives as sharp, bright, punctiform. The application of the term 'pressure' to this sensation seems unfortunate, since the prominent component in the 'pressure' of ordinary speech is undoubtedly not

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[1] *Cf.* W. Wundt: *Physiologische Psychologie*, 1902, I, 400.

[2] The existence of a range of fairly unpainful pain sensations, corresponding to the response of the pain endings to a lower range of stimulus intensities than is usually thought possible, is of interest more especially in relation to the origin and explanation of organic sensation, in connection with which it will receive fuller discussion in a later article.

[3] The question of the uniqueness of its quality is reserved for later discussion.

the granular quality but the deeper element which we have just thrown out of the discussion. The expression 'light pressure' or 'touch' would seem to be the least ambiguous.

Careful examination of Goldschieder's monographs on cutaneous sensation reveals many unexpected congruities between his observations and our own. In the interests of the doctrine of specific energies, we find many qualitative phases of cutaneous sensation carefully examined and recorded. Among these are certain trenchant descriptions of pressure-spot stimulation, phrased in terms which are a tacit recognition of the complexity of the so-called granular sensation. The characteristic feeling is "kein eigentlich punctförmiges" but "als ob man auf ein feines Körnchen Staub druckt";[1] "wenn man auch nicht sagen kann dass er flachenhaft irradiiert wie bei den Temperaturpuncten, so ist er doch breiter als zum Beispiel die stechende Empfindung, welche in der That als punctförmig zu bezeichnen ist. Man könnte das Druckpunktgefühl vielleicht als ein dem punctförmigen nahekommendes aber volleres Gefühl bezeichnen."[2] The discovery which Goldscheider here believes himself to have made is that the pressure sensation, even in the last analysis, is not punctiform but retains its characteristic pressure quality, *i. e.*, its spatiality, solidity. This solidity, of course, we should maintain to be, not a characteristic of the ultimate unit, but a correlate of the complexity of the granular pressure itself.—

This point comes out more clearly in the contrast drawn between pressure-point stimulation and that of intermediate regions. On the latter the sensation is either "ein mattes unqualificiertes," or on certain points, with sharper stimulus, "mehr stichartig, das heisst, ohne Schmerz, aber auch ohne die qualificierte Tast- und Druckempfindung, dünn, inhaltlos." This "stechende Gefühl" of the intermedite points, which with increased intensity easily passes over into a painful prick, corresponds evidently to our bright sharp sensation, and the points on the skin to our original contact points. These points were at first distinguished by Goldscheider from the pain points proper, but the distinction was later recognized as one of limen only, and the two were grouped together and referred to the *Gefühlsnerven*. This setting-off of *Gefühlsnerven* from *Drucknerven* corresponds, according to Goldscheider, to a functional rather than a qualitative differentiation. It represents a separation of function in the perception of the objective as over against the subjective aspect of experience. The *Gefühlsnerven*, branching everywhere in the skin, render

[1] Gesammelte Abhandlungen, I, 1898, 187.
[2] *Ibid.*, 191.

it sensitive to mechanical stimuli in general and to intensive stimuli in particular. The specific pressure-nerves, on the other hand, are especially adapted to the perception of low intensities and intensive differences, and of small differences in locality.

The pain into which this "punktformige" quality, produced by the stimulation of the *Gefühlsnerven*, goes over with increase of intensity is described as "ein schmerzhaft stechendes, welches durchdringend, lancierend, und meist in Moment des Entstehens am stärkesten ist, um trotz Fortdauer des Reizes schnell zu erlöschen." The painful excitation of a pressure point, on the other hand, makes a more powerful impression on the sensorium, lasts as long as the stimulus, with a tendency to grow stronger rather than to weaken.[1] The passage in this case of the granular pressure into pain is described as a transition into a "ziehendes, druckendes, quetschendes Schmerzgefühl," as over against the "stechartiges lancierendes" of the intermediate points; a distinction similar to that made by one of our observers.

According to Goldscheider, then, there are two sets of points on the skin, of which the one may yield a *Beürhrung* (or *Kitzel*)-*Druck-Schmerz* continuum, the other a *Berührung-Stich-Schmerz* series, the former mediated by *Drucknerven*, the other by *Gefühlsnerven*. The precise relationship of the *Druck* and *Stich* qualities (both of which are said to shade into pain at the upper end of the continuum) is left uncertain. Pain, when not too intense, is said to have "gewisse verwandtschaftliche Beziehungen zu Berührungs- und Druckqualitäten," but the anomaly of its appearance at the summit of two disparate continua is left unexplained (unless the hypothesis of gray-matter conduction in either case be taken as an explanation). Our interpretation, while doing justice to the shades of similarity and difference noted by Goldscheider, would simplify explanation by reducing the distinction between the *stichartiges* and *körniges Gefühl* to one of complexity and not of quality, and by assigning certain aspects of so-called pressure to deeper, non-cutaneous endings.

A word may be added in conclusion, in support of our proposed simplification of the pressure concept. It may be objected that to push analysis beyond the normal functional unit, *i. e.*, granular pressure, is a work of supererogation; a sense contents is practically unitary as long as it evades analysis, and this the granular pressure normally does. In reply we can only urge that a unit which may go to pieces in the hands of the experimenter is, under any conditions, an unsatisfactory basis of op-

[1] *Op. cit.*, 198.

erations. To be useful in investigation, a unit must be not only unanalyzed but unanalyzable, as our difficulty in identifying tickle and electrically excited sensations with the pressure quality has shown. Further illustrations of this point may be drawn from the sphere of semi-subjective sensory complexes, cutaneous and organic, where the danger of mistaking what we may call (in analogy with *Klangfarbe* in tones) the form-color or quality of a sense experience for ultimate qualitative contents, *i. e.*, for a new quality, is constantly present. In accordance with our original purpose, we insert under the next heading a description and tentative analysis of some of the simpler of these complexes as encountered in the study of pressure.

2. *Form-Color: The Apparent Metamorphosis of Sense Qualities through Elaboration.* For the study of apparent alterations of quality through reduplication and complication, stimulation of the skin with the interrupted current is invaluable, affording, as it does, from moment to moment transition phases in the formation and disintegration of fusions and complexes. The conception of granular pressure as a fusion of points on the verge of disintegration because of temporal or intensive oscillations, and the possibility that just as granular pressure represents a half-fusion of sharpness-units so dull areal pressure represents a smooth fusion of low intensities of the same units, were suggested by the use of the interrupted current.

a. The Qualitative Validity of the Term 'Dull.' Even if the dull component of our original granular pressure arises from a distinct set of deeper endings, its qualitative uniqueness is by no means assured; it may represent merely a blurring together of sensation-units similar to those of our superficial tactual quality. Attention was drawn to this speculation by the following observations. First, the after-image of a "fizzly" or ticklish pressure, *e. g.*, the sensation resulting from a light thrust on a swollen and reddened area or the palm of the hand, often approximates the dull, "cottony" unanalyzability of our deep pressure. Secondly, with continued faradic stimulation of the skin, a certain zone of sensation tends to lose its prickly intermittent character and to approach the dull areal pressure quality. A similar experience follows the holding of a finger against the vibrator of an inductorium or the end of a tuning fork; at a certain point of fatigue and dulling of the original bright intermittent sensations, the disparate vibrations become more and more indistinguishable, until finally the complex falls just below the level of analysis and a dull smooth areal impression succeeds. A similar gradation into dull pressure occurs in the dying away of the tingling of a limb which has

been "asleep" into either a massive dull sensation or a mass of soft blurred barely analyzable fluttering sensations. That is, the appearance of a dull massive contents marks apparently not the transition to another quality but a stage in the amalgamation of otherwise punctiform sensations. Hence the dull pressure of ordinary speech, even if mediated by deeper endings, conceivably represents not a quality peculiar to them, but a fusion of sensations like in kind to our elementary tactual quality, but of moderate intensity (the endings being secured against violent stimulation) and continuous, not irregular. That is, dullness may represent a certain "form-color" rather than a primitive quality: the terms massive, solid, may refer less to intrinsic qualitative attributes than to associated imagery, such as the hazy visualization of the tridimensional objects by which the displacement of deep pressure is ordinarily effected, or to a vague consciousness of the spatiality of the region of the body affected.

b. Sharpness. A similar query as to the status of the adjective 'sharp' rose in connection with the characterization both of tickle and of electrically excited sensation. The possible interpretations fall into two main categories: either sharpness represents an original quality, hardly susceptible under any conditions to clouding and blurring; or sharpness is simply a function of the isolated appearance of a sensation, hence capable of attaching itself to any quality indifferently. The latter possibility is itself susceptible of various explanations, *e. g.*, the following:

(1) Sharpness or pointedness may represent the opposite or absence of extendedness. Extension or surface may be an associative factor, visual or kinæsthetic, set up only when the stimulation affects two or more points. Hence pointed or sharp would be the equivalent either of absence of visualized extent, or of visualization of a single point.[1]

(2) Sharpness may denote the absence of the fringe or halo of weaker sensations usually present as a result of the diffusion of stimulation to adjacent endings. In other words, it may represent not an attribute of sensation but a relation to other contents, *i. e.*, definiteness of outline, or sharpness of differentiation from the context.[2]

(3) Sharpness, in its cutaneous application, may signify merely abruptness of rise and fall of sensation, clean temporal definition; correlated in tickle with the oscillating intermittent

[1] We have no intention here of raising the question of the spatial attribute as intrinsic to the tactual sensation. The point at issue is merely the presence or absence of an appreciable *surface*.

[2] Sharpness in this sense is practically equivalent to 'distinctness' in the usage of Wundt.

character of the sensations, in contact or light touch with abruptness of appearance and disappearance, in contrast with the gradual swelling out and dying away of a dull, heavy pressure.

(4) Lastly, sharpness or brightness may connote merely a vividness, penetrativeness, correlated with the concentration of attention on a single process as against its customary dispersion over a total contents representing several processes. For example, the sharpness or vividness of a pain sensation may be largely a function of its monopoly of the focus of attention.

As the result of many observations the decision was finally reached that the contents described as sharp is really the *typical tactual quality*, capable, to be sure, of definite dulling or brightening, under the influence of variations either in intensity or in any one of the four conditions above listed.

c. Principal Types of Form-Color in Tactual and Kinæsthetic Sensation. Parallel with the above study, the working over of a number of introspections on sensory complexes akin to the organic (accumulated in connection with the analysis of granular pressure and tickle) was attempted. The plan adopted was to select certain of the specific sense-experiences arising from the excitation of the skin and immediately underlying tissues, to examine the transitional phases, and to determine how many of the differences accepted at first sight as ultimate resolve themselves on analysis into differences in the consolidation or co-ordination of the component sensations. The gooseflesh shudder, the tingling following a sharp rap on the hand, the "waking up" of a limb which has been asleep, and the ordinary muscle, joint, and strain sensations were carefully observed in their waxing and waning,[1] with the results recorded below.

The sensation of stretching or tautness of the skin, common to certain stages of the gooseflesh shudder, electrical excitation, and the after-phases of the sensations associated with tickling and a smart blow on the skin, we were at first inclined to regard as a specific quality, having its origin in the sensory endings in the plain muscle fibres of the skin and excited by the contraction of the latter. Since, however, this unique sense-experience was observed always to accompany the dying away of an extended field of sharp intermittent sensations, the suggestion forced itself on us that this also was a case of peculiar

[1]This procedure approaches as nearly to the "synthetic experiment" as our lack of knowledge of the precise excitatory conditions in the various phenomena would admit. It is, of course, open to objection in that apparent gradations in a homogeneous complex may really mean the gradual swamping of one quality through the emergence of another. The error thus introduced we believe, however, to be inconsiderable with practice in analysis.

"form-color," the mode of appearance in consciousness of an extensive incorporation at a certain stage of consolidation or blurring. That is, the tension in the gooseflesh complex represents a closely woven web of superficial sensation, from which deeper components are lacking. This unique tactual formation, excited ordinarily only by stretching of the skin, serves as a functional unit or symbol for a single objective fact, *i. e.*, tension, and has thus won for itself a solidarity or unitariness which enables it to pass itself off as a specific quality.

A second set of sensations which seemed likely to defy analysis was that of strain. The strain complex, as it occurs in association with vigorous contraction of the skeletal muscles, is distinctly localizable in the tendinous regions near the attachments of the muscles. In comparison with the sense contents of either articular or muscular sensation, it is in its higher intensities unique, and might be characteristically described as suggestive of an iron tension or rigidity. This characteristic contents was, however, observed to occur in other contexts than that of muscular tension or exertion, and in various regions. In the reflex excitation of the salivary glands by sour liquids or odors, in general physical fatigue, in neuralgic headache, in a certain intolerable phase of the tingling of a foot or arm which has been asleep, and after prolonged application of icy objects to the skin, very fair imitations of the tendinous strain quality occur. It seems improbable that in all these cases specific end-organs of strain are affected; and indeed the observations on the transitional phases and gradations in electrical sensations, tingling, and glandular excitation, all go to show that strain, like stretching, represents the welding of a certain group of sensations of the bright tactual quality.[1] As sense contents it differs from stretching mainly in its greater bulk and intensity, and possibly in a greater compactness and closeness of union between its individual members. Here again the unitary functioning of the complex (in tendinous strain) has furthered the specific quality deception.

Thirdly, the sensory contents of the numbness following fatigue or stoppage of the circulation was examined under different conditions, and was found on close scrutiny often to resolve itself into a multitude of dulled unsteady shadowy sensations, giving a total impression of massivity, tridimensionality, but possessing neither the smoothness nor the stability of the ordinary dull pressure experience.

Lastly, the "circulatory" complex or tingling in "pins and needles," the uniqueness of which as conscious contents is

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[1] Whether mediated by superficial or deeper endings in any given case is hardly determinable.

unquestionable, was found to present a close parallel both to intermittent electrically excited sensations and to the goose-flesh shudder which, under close observation, is plainly pricking at a certain stage. If we accept the electrically excited sensations as a peculiar disintegration of the granular pressure, and recognize their close similarity to the so-called circulatory sensations, the necessity for forming a special category for the latter lapses, and their uniqueness may be construed as residing not in the individual, but in the aggregate or collective aspect of the complex, the 'form-color.'

In the light of the above introspective evidence it would seem possible to classify these apparent cases of specific quality as complexes or extensive incorporations of varying degrees of consolidation. Three distinct types, representing as many degrees of fusion, may be deciphered. These may be schematically indicated as follows:

(1). The most stable: below the limen for analysis. Two sub-types may be distinguished on the basis of intensity and disintegration: *a.* a smooth fusion or blurring-over of sensations of weak intensity, with incentive to analysis lacking: illustrated by dull pressure or muscular fatigue; *b.* a firm fusion, of considerable intensity, with no tendency to disintegration, illustrated by the sensation of strain.

(2). Of less stability: much blurring and weak intensity, but a slight irregularity or intermittence in single members of the group, which brings the complex just up to the limen for analysis: feeling-tone uncanny, "nervous." Illustrations: numbness, some shades of tickle, the after-image of pressure, the response of a fatigued pressure spot.

(3). Noticeable instability: kaleidoscopic changes; obviously a plurality of sensations, whose intermittence and degree of intensity are both favorable to analysis. Pricking, tingling, and electrically excited sensations are examples.

To those inclined to posit definite boundary lines between pain, pressure, joint, muscle and strain qualities, this attempt to reduce the whole *Gefühlssinn* to a common denominator will seem extreme. Wundt is perhaps at present the sole representative of the school who found in 'touch' a category sufficiently comprehensive for all mechanically excited sensations, external or internal. The tendency of the present is in favor of multiplication of qualities, and the effort to push analysis behind the accepted psychological atoms or indivisibles in the interest of a further simplification of qualities will probably be deemed as fanciful as Leibniz' or Spencer's postulation of unnoticeable part-processes. The dissection here carried out, however, is not purely hypothetical, but was effected in almost every case directly by introspection. Further, while it is un-

deniable that the unlikeness of the experiences cited is functionally more significant than their ultimate homogeneity, work in the new sphere of organic complexes would have been almost impossible without the aid of the concepts of combination or complication thus acquired. From the point of view of method, therefore, such distinction is amply justified.

These supplementary studies have further shed light upon the conditions and significance of certain judgments involved in the description both of tickle and of organic sensation. Before passing to the discussion of tickle, a brief section will be devoted to this subject.

3. *The Sensory Basis of Certain Judgments: Localization, Intensity, Area, etc.* The point here raised is not that of the accuracy of external reference and projection in general, but merely of the reliability of certain judgments as a basis for inference regarding the precise origin and explanation of various phenomena,—tickle, tingling and the like. The question may be formulated as follows. To what extent do apparent localization, intensity, and volume depend directly and unequivocally on sensation attributes and differences? How far are they mediate rather than immediate perceptions, dependent on certain intervening assimilatory ideas and associations,—and thus, for our purposes, open to suspicion? Prolonged introspective study seems to show that visual imagery and stimulus-reference, correlated with variation rather in sensation pattern than in the immediate sensation attributes, form in many cases the important factor.

(*a*) *Intensity.* In the intensity judgments, heavier and lighter, stronger and weaker, variation in the total sense contents enters into the judgment as follows. Even with a punctiform stimulus, variation occurs in the amount of diffusion of stimulation superficially and in the third dimension. Hence the observer frequently reports that the extent to which weaker outlying sensations, or a deeper component, dull pressure or ache, are present forms the basis of his judgment; while visualization of the amount of surface indentation often forms an intermediate link. Interpretation of the stimulus is, therefore, commonly confused with sensation intensity pure and simple. Only thus could the common error have arisen that tickle is characteristically weak and faint as compared with contact and pressure sensations; its customary stimulus is weak, but its own intensity is really in keeping with the energy of the response which it excites.

(*b*) *Spatiality; Extendedness and Solidity.* The distinction drawn by our observers between pressure and touch as tridimensional and bidimensional resolved itself, in the majority of

cases, into a matter of visual association. Bright sensations of a certain intensity and massing are almost indissolubly associated with superficial stimulation, *i. e.*, with the surface of the skin. Whatever is closely associated with visualization of the skin must get thereby (if it does not have it originally) area, bidimensionality. Further, a certain pattern of dull massed sensations is associated in experience with the partial displacement of some fleshy portion of the body, and is never actually projected to the surface. Hence the tridimensionality or solidity of muscle or deep pressure sensations is apparently borrowed from the suggestion of volume in the associated imagery. The significance of the term 'point,' on the other hand, seems to connote an almost total absence of body, of spatiality, and is associated with a lack of projection or objectivity.

(*c*) *Localization in the Third Dimension.* Tickle is sometimes localized "just under the skin," dull pressure always. Secondary pain seems to spring up from below, pins and needles seem to dance to and fro in the third dimension, penetrating toward the surface and retreating. Evidence in favor of assigning these sensations invariably to deeper lying endings is, however, lacking. The "depth" perception is dependent on certain habitual principles of objective reference, which, while misleading for our purposes, in the majority of cases work satisfactorily to the organism. The important question ordinarily is not in what layer are the nerve endings affected, but what is the situation of the exciting cause: and the customary spatial interpretation represents with sufficient accuracy the answer to this question. Itch, secondary pain, pins and needles and the like are conditioned immediately by processes within the skin. The immediate incentive to inward localization in these cases is, doubtless, furnished by temporal inertias and irregularities which experience has correlated almost invariably with internal rather than external stimulation.

(*d*) *Movement.* The apparent advance and retreat in the third dimension in "pins and needles" suggest the basis of certain illusory judgments of movement. It seems improbable that the apparent travelling or creeping on the surface for minute distances of the faint tickle-sensations excited by punctiform excitation actually represent either the successive stimulation of different nerve-endings or central irradiation. It is not unlikely that, in analogy with the judgment of apparent movement in pins and needles, slight oscillations in intensity or even irregularity in the successive response of the endings belonging to a single pressure point are interpreted as linear movement on or just below the surface.

(*e*) *Projection in General.* The conditions which determine the completeness of development of projection or objective ref-

erence in any case—*e. g.*, to the body wall, to the body surface, or beyond the surface in visualization of the stimulus—have been indicated under the preceding heads. The conditions which prohibit the passage of a sensory complex beyond the bounds of the subjective, preserving it as an affective rather than a sensory phenomenon, will be discussed at more length in connection with tickle.

SECTION B. TICKLING

HISTORICAL: THE PROBLEM OF TICKLE

The peculiar affective reaction excited by a light touch has long been a matter of remark among psychologists and biologists alike. The fact that a feeble stimulus evokes such an exaggerated commotion in consciousness, whereas a heavier one remains indifferent, presents a paradox to psychophysics which has never been satisfactorily explained. The observation that this affective disturbance, which is sometimes pleasant, sometimes uncannily unpleasant, is in some individuals entirely lacking only adds to the difficulties of explanation.

The older psychologists, of semi-physiological bent, threw the burden of explanation upon the obscure concept of "central irradiation," and were content when they had classified tickling with the *Gemeinempfindungen*, the unprojected, ill-analyzed masses of bodily sensation, which function as subjective and affective rather than objective and perceptual. Later, a biological interpretation was attempted, and various speculations were advanced as to the significance in the past history of the race of the reflex vigor and affective response now associated with tickling. Lastly, experimentalists have attacked the problem, and sought to discover in the histology and physiology of the skin, and in the immediate sensory contents of tickle, the peculiar conditions by which it is set apart from cutaneous sensation in general. It is with this latter phase of the investigation that we are here immediately concerned, believing that it alone can afford a satisfactory basis for the explanation of the peculiarities of the tickle consciousness as a whole.

1. Tickle as centrally conditioned, non-projected sensation (Gemeinempfindung *or 'feeling'*).

This type of explanation originated with Weber, but has since undergone elaboration at the hands of Wundt, von Frey and others. The uniqueness of tickling, as of the related experience of gooseflesh, resides, it is held, in the eccentricity of its temporal course (inertia of appearance and disappearance), and in its tendency to diffusion or irradiation. In consequence of these peculiarities, exact correlation between stimulus and

sensation is impossible, and tickle remains among the sensory experiences which fall short of projection beyond the physical self, *i. e.*, among the *Gemeinempfindungen*. With organic sensation proper this failure of external reference may be ascribed to the internal or subjective nature of the stimuli. Tickling, in spite of the external character of its primary stimulus, presents a parallel case; for its actual constituents are secondarily excited sensations (*Mitempfindungen*, in the terminology of Quincke), the temporal and spatial course of which corresponds only remotely with that of the original stimulus.

The mechanism by which these secondary sensations are excited is explained by E. H. Weber,[1] and after him by O. Funke,[2] H. Quincke,[3] W. Wundt,[4] and M. Dessoir,[5] as purely central, an irradiation confined to the sensory centres or ganglia, and referred to the surface only through a special form of excentric projection. Another set of writers, B. Bourdon,[6] M. von Frey,[7] O. Külpe,[8] A. Allin,[9] C. S. Sherrington,[10] and in some passages Wundt favor irradiation from sensory to motor centres. According to the latter theory, diffusion or irradiation, while centrally conditioned, takes effect peripherally, through the agency either of vasomotor changes or of the contraction of the unstriated muscle fibres of the skin. Among the supporters of the vasomotor theory, von Frey and Bourdon admit the possibility that tickle constitutes a specific sense, distinct from pressure, with endings of its own in the skin or vessels, Bourdon maintaining that pleasure itself is a diffuse sensation of tickle. Külpe, on the contrary, holds that tickle contains no new element, but represents a combination of the qualities of pressure and temperature characterized by rapid alternations of contents and intensity, and closely related to tingling and itch. Wundt, in the Human and Animal Psychology, suggests that sensations from the reflexly excited unstriated muscle layer in the skin combine with the feeling of tickling, lending color to the total impression. Sherrington goes further, regarding tickle as "a peculiar psychic elaboration of tactual and muscle impressions,

[1] E. H. Weber: Der Tastsinn und das Gemeingefühl, 1851, 565-6, 578-9.
[2] Hermann's Handbuch der Physiologie, 1879, III, 313.
[3] Über Mitempfindungen, *Zeitschrift für klin. Medicin*, XVII, No. 5, 1889, 443.
[4] Grundzüge der physiologischen Psychologie, 5te Aufl., II, 42.
[5] Über den Hautsinn, *Du Bois-Reymond's Archiv*, 1892, 237.
[6] La sensation de plaisir, *Revue philosophique*, 1893, XXXVI, 225.
[7] Untersuchungen über die Sinnesfunctionen der menschlichen Haut, 1896, 217.
[8] Outlines of Psychology, 89.
[9] On Laughter, *Psych. Review*, X, 1903, 307-8.
[10] E. A. Schäfer's Textbook of Physiology, 1900, II, 976.

the latter arising from the reflex activity of the unstriated muscles of the skin, pilo-motor, sudorific and vasomotor."

II. Tickle as giving rise to affective disturbances and reflexes. In this connection three different explanatory factors have been advanced, the summation of intensities, psychical attitude, and the inheritance of adaptive reflexes.

Summation of intensities. According to E. Kroner,[1] summation of intensities in sensory centres and nervous excitability furnish the explanation of the disproportion between the intensity of the stimulus and the resulting disturbance in consciousness. Allin[2] favors motor rather than sensory summation, quoting Stirling's observation that reflex contractions occur only from repeated shocks in nerve centres. Tickle, he suggests, may represent a sudden convulsive hyperæmia (presumably cerebral) entailing an explosive motor discharge, as contrasted with the diffused hyperæmia of a steady pressure. A possible cue to the difference in the reflex effect of a light and heavy touch is also offered in Lauder Brunton's suggestion that the latter may stimulate two sets of nerves which counteract or inhibit each other.

Psychical attitude. A second possibility has been advanced by C. Darwin[3] and approved by K. Groos,[4] E. Kroner, J. Sully,[5] and others, to the effect that the reflex disturbance of tickling (as evoked in movements of laughter, of withdrawal, and of either protective or purposeless character) is conditioned not by the peculiar characteristics of the initiatory sensation, but by the psychical attitude or disposition upon which such sensations are superimposed. Darwin finds surprise or novelty the all-important factor, and thus reduces the tickle reflex to an expression of emotion. He supports this conclusion by the facts that the parts of the body most sensitive to tickle are those not commonly touched or touched in tickle in an unusual fashion, that the part to be tickled must not be known, and that one cannot tickle oneself. According to Sully, the mental agency conducing to tickle is in the nature of shock or relief: a sense of the unknown or unpredictable in the situation, followed by the recognition of the merely playful character of the attack. Laughter itself is probably a physiological device for

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[1] Das körperliche Gefühl, 1887, 164, 200 ff. E. C. Sanford also classes tickle as a summation phenomenon, citing in evidence the fact that a light touch with the prong of a tuning fork is often ineffective when a vibrating prong is immediately productive of tickle. Experimental Psychology, 1898, 18-19.

[2] On Laughter, *Psych. Rev.*, 1903, X, 306 ff.

[3] The Expression of the Emotions, 1872, 201-2.

[4] The Play of Man, 1901, 165.

[5] Essay on Laughter, 1902, 59-61.

the relief of cerebral strain. The stimulation of deeper lying endings is not essential.

A third interpretation regards the reflex response as *the survival from some ancestral instinct*, the significance of which is not perfectly apparent. Various possibilities have been suggested. According to the first, tickle is a relic of a primitive contact-sense, which existed before the development of the senses of 'anticipatory touch,' and necessarily possessed a high dynamogenic value and lively reflexes; when danger was announced only by contact, with great suddenness, strong reactions of escape or resistance were necessary. G. S. Hall and A. Allin[1] and Bronson[2] support this view, the former citing in evidence the fact that the parts of the body most sensitive to tickle are those most vulnerable or most open to attack, *e. g.*, the soles of the feet, palms, and throat, in contrast with the shoulder-blades, calves and thighs. The suggestion is also made by Hall and Allin, and approved by Sully, Sherrington, and others, that the necessity for protection against the incessant attacks of insect parasites has played an important rôle in the maintenance of this sense. A similar explanation is put forward by L. Robinson,[3] according to whom tickle is distasteful or agreeable according as it represents a warning against insect foes or possesses some obscure associations with caressing movements dating back to an early stage of existence.

III. Tickle as a Sense Quality. (*a*) *Relation to Touch.*—In the general literature, tickle is identified more or less closely with contact, touch, or pressure, with itch or with the goose-flesh shudder. Bronson and Dessoir[4] separate touch or contact from pressure, assigning tickle to the former sense; Goldscheider,[5] Kiesow,[6] and L. Herrick[7] classify tickle with pressure, limiting it, however, to cases of a peculiar functioning of the specific pressure organs; Külpe[8] and Wundt, while iden-

[1] The Psychology of Tickling, Laughing and the Comic: *Am. Jour. of Psych.*, IX, 1897, 14.

[2] *The Medical Record*, xxviii, 425.

[3] *Cf.* Tickling: H. Tuke's Dictionary of Psychological Medicine.

[4] *Op. cit.*

[5] Gesammelte Abhandlungen, i, 1898, 45.

[6] *Zeitschrift f. Psych. und Phys. d. Sinnesorgane*, xxxv, 1904, 240-1.

[7] Corollaries of Neurological Discoveries, *Jour. Comp. Neurology*, 1897, 160.

[8] Outlines of Psychology, 89. According to Külpe, tickling often accompanies the weak or intermittent stimulation of a pressure organ. Since a gentle pressure or blowing on the skin is found to be often followed by increase in arterial pressure which intensive or even painful stimulation is unable to effect, it seems probable that "certain processes in the cutaneous vessels serve as the substrate of the impressions of weak pressure and more or less vivid heat which occur in quick alternation in both tickling and itching."

tifying the essential constituent as tactual, recognize the presence of an added element, reflexly aroused by vasomotor or other changes, in the form either of warmth or of muscle sensations.

The distinction drawn by Dessoir between tickle and pressure is really, however, perceptual rather than qualitative and is based upon the absence of externalization in the former sensation; he does not posit a distinct set of endings for his contact-tickle sense. Kiesow identifies tickle unequivocally as a pressure or *Tastempfindung* arising from the direct excitation of a pressure organ, intimating, however, that the stimulation must be accomplished through the medium of the hair, *i. e.*, by vibratory or intermittent excitation. Herrick, though failing to work out any definite conception of the relation between tickle and pressure or touch, explains the diffuse irradiating character of tickle by reference to the anastomosis between tactile corpuscles noted by Dogiel[1]. Goldscheider,[2] while ultimately falling back on a variation of the *Gemeinempfindungen* concept, attempts to explain the peculiarities of tickle by reference to the mechanism and characteristics of the sense department to which it belongs (pressure in his system). While his observations are merely incidental to the investigation of pressure, and are limited to tickle sensations initiated by punctiform stimuli, they bring to light many important facts. Of these the following, since disputed by Alrutz, are noteworthy. First, tickle may appear simultaneously with the sensation of touch which frequently attends it, *i. e.*, a delay between stimulation and the appearance of the sensation is not characteristic. Secondly, there are no specific tickle points; exploration of the skin with a bit of cork discloses tickle only over the *Druck* or *Schmerz* points. Other observations mark the correlation of tickle with weak mechanical or electrical stimulation of the skin: its liability to fatigue; its high affective value and qualitative indefiniteness; its long after-image and the tendency of this after-image to fade indistinguishably into touch; the peculiar distribution of tickle sensitivity, and the inverse relation of the tickle range of any area to its pain and pressure sensitivity. From these observations, Goldscheider draws the conclusions that tickle represents not a secondary sensation, but the direct result of weak mechanical stimulation of the pressure endings, a belief in which Kiesow alone seems to support him.

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[1] *Cf.* Wundt's speculation on the function of the Krause endbulb, *op. cit.*, 13, for a similar suggestion.
[2] *Cf.* Die specifische Energie der Gefühlsnerven der Haut, 1884, 45, 46, 81, 82; and Neue Thatsachen über die Hautsinnesnerven, 1885, 202-4.

The precise manner in which Goldscheider conceives the relation of tickle and pressure differs slightly in the two monographs cited. In the first he maintains that the qualitative continuum of touch or pressure is threefold, comprising tickle, pressure and pain, each representing a special shading, as it were, of the elemental quality. For the production of tickle neither successive stimulation of different end-organs nor repeated stimulation of the same end-organ is necessary; tickle is the "eigentliche specifische Empfindung der Tastnerven" and arises normally from any single weak stimulation of the end-organ. A single stimulation probably gives rise, however, to a whole series of successive disturbances or excitations within the sense-organ. Weak mechanical stimulation is probably the condition most favorable to the uniformity and continuance of this series of excitations, which is perceived as tickle and is pleasant. When the mechanical shock is sufficient to affect the surrounding tissues, the commotion there produced reacts upon the end-organ and either damps off its vibrations or disturbs their regularity. This new state of affairs is transmitted to consciousness in terms of indifferent touch.

On the basis of this hypothesis the correlation of tickle with weak stimulus intensities and its long after-image are readily explained. Two points, however, remain obscure. First, does Goldscheider regard the oscillation ascribed to tickle as a central and conscious matter, or as peripheral and neural merely? Secondly, what is the relation to the system of the apparent irradiation or diffusion of sensation in tickling, which (apart from one casual reference to secondarily excited sensation) is passed over in silence?

Further, it is evident that, from the point of view of the doctrine of specific energies, the association of a three-fold qualitative continuum with a single set of nerve endings has a certain inconsistency. The appearance of tickle as a phase of the *stichartige* and the *körnige* quality in turn, in its character as an exponent of either *Druck* or *Gefühlsnerven*, offers yet another anomaly. The modification of the conception of tickle which appears in Goldscheider's second monograph was apparently framed in recognition of these difficulties. Whether or not it escapes them is matter for discussion.

According to this later view, the distinction between touch and tickle is not merely a matter of qualitative shading but of degree of objectivation or externalization also. Tickle, which is held to arise simultaneously with contact or touch, and preferably but not invariably with the weaker phases of intensity, is not an independent qualitative co-ordinate of the latter, but something *intrinsic to every touch sensation*, an affective shading, as it were, through which the latter is sensed. As in the sphere

of vision, the less distinctly the outlines of an object emerge, the greater is the prominence of the color in which it is clothed, so in tactual sensation, whatever acts to help or hinder projection and definition acts inversely to suppress or heighten the prominence of the affective shading or tickle.

In the earlier monograph, regional differences of sensitivity were referred to the physiological fact of variation in the *Kitzel-*, *Tast-*, and *Schmerzbreite* of the end-organs on different areas; on the specialized tactual surfaces (finger-tips, etc.), the point in the intensity scale at which the quality of tickle passes over into that of touch is very low, on others it is correspondingly high. In the later article, differences in sensitivity are explained on the basis of the use or disuse of the area in question as a perceptive surface, and the fixity or absence of the habit of external reference. Tickle is not simply the sensory effect of the weakest excitation; it is the unprojected, unreferred remnant in every tactual sensation. At first glance this would seem to signify that the simplest tactual contents, as such, possesses both an objective and a subjective aspect, the obverse and reverse, so to say, of the same bit of sensation. Frequent association of the sensation with external stimuli develops the objective at the expense of the subjective or tickle aspect; the unprojected tickle remnant (*Kitzelgefühl*) dwindles.

This as it stands is not unintelligible; although, strictly taken, it is not easily translated into the terms of any psychological system. But Goldscheider's actual understanding of the matter seems to be even more intricate. The simplest touch sensation is not only functionally but is also qualitatively twofold. The split in function runs parallel to a differentiation of contents; tickle is that portion of the contents which never undergoes external reference, the feeling aspect; pressure or touch is the objective or perceptual aspect. With the development of the objective, touch or pressure aspect of the contents, the subjective aspect does not actually dwindle but simply drops into the background, is swamped in the objective. Hence, it is that the feeling of tickle is not absent but is merely in abeyance when tactual surfaces such as the fingertips are lightly stimulated.

This doctrine, taken in detail, is obviously quite as difficult to reconcile with accepted theories of nerve action as was the earlier version. It is, however, highly suggestive, and in the light of further observations a revision of it may be possible.

(*b*) *Relation to Itch*—A close relation between tickle and itch is noted by certain writers. Goldscheider himself suggests that itching is only some more intensive form of tickle. Von Frey [1] is in favor of identifying the two. He refers them to

[1] *Op. cit.*, 217; and Beiträge zur Physiologie des Schmerzsinnes, *Berichte d. math.-phys. Classe d. kön. säch. Gesellschaft*, 1894, 192.

certain vasomotor reflexes set up by touch and thus explains the delay, lack of exact correspondence with the stimulus intensity, liability to fatigue and disappearance of such sensations when the sense of pressure suffers merely a raising of the limen. This conclusion is based largely upon the exclusion from the category of tickle of the weak punctiform sensations recognized by Goldscheider, but classed by von Frey as indifferent *Berührungsempfindungen*. Von Frey expressly restricts the term tickle to the lively, lasting, irradiating sensations, with strong unpleasant tone and a tendency to awake reflexes, which appear in the train of mechanical excitation.

Richet[1] distinguishes tickle and itch only by the external or internal origin of the initiatory stimulus; at the same time, he maintains the close relationship of tickle and touch. Alrutz[2] has attempted, perhaps, the most extended investigation of tickling, but his results also are biassed by a tendency to limit the phenomena under consideration to the semi-painful or itchy phases of tickle. This limitation leads him ultimately to identify tickle with Goldscheider's secondary pain, and to erect out of the two experiences a new sense distinct from primary or pricking pain. The observations which he cites record the results of punctiform stimulation of the cornea, conjunctiva, hard palate, lips, nostrils, and volar surface of the lower arm. Tickle is reported from all these areas, with transitional forms grading into itch from the margins of the lips. The application of a moving stimulus (camel's hair brush) to the lips, forehead, region under the eye, palm and sole of the foot (in the last case a cylinder covered with cotton was used), gave similar results; on the palm the weakest perceptible stimulation gave only touch, and tickle required a slightly higher stimulus intensity. The tongue is also found to respond with tickle when an intermittent stimulus (vibrating fork) is applied. The distinction between deep-seated and superficial tickle (made by Robinson, Sanford and others) is held by Alrutz to be practically non-existent for introspection; the tickle produced by heaviest stimulation of the sole of the foot, apart from the tendency to excite reflexes, differs not at all from the superficial tickle elsewhere.

On the basis of these and other observations, Alrutz concludes that tickle represents a peculiar quality, allied to itching and the secondary pain of Goldscheider. Its distinguishing characteristics, irradiation, diffuseness, indefiniteness and unpleasantness, separate it alike from pressure and pricking pain. For this quality a distinct set of nerve-endings must be posited;

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[1] Ch. Richet: *Chatouillement*, Dictionnaire de Physiologie, 1900.
[2] S. Alrutz: *op. cit.*

Thunberg's assumption that the same nerve apparatus, directly or indirectly stimulated, mediates primary and secondary pain in turn is untenable. For this new quality the free intra-epithelial endings present the most likely substrate. The more superficial position and less highly developed structure of these endings explain at once its low limen, inertia, and susceptibility to fatigue.[1]

The following points are offered in support of this hypothesis. (1) Tickling and itching can be obtained only between the pressure points. (2) The limens for tickle, touch, and primary pain are usually different. (3) The latent period for the first is longer. (4) The quality of tickle is nearer that of itch than of pressure, especially in its tendency to irradiation and the production of reflexes; the difference between tickle and itch is mainly one of degree, though the frequent presence of touch sensations with the former usually serves to distinguish them further. (5) Cases are recorded of analgesia without anæsthesia but with loss of tickle. (6) The areas insensitive to tickle and to itch are identical. (7) Areas where pressure and pricking pain are poorly developed possess a strong sensitivity to tickle.

To Alrutz' hypothesis of a specific nerve-energy peculiar to tickle and itch Thunberg[2] opposes the view that these peculiar sensory experiences are completely explicable as modifications of the familiar qualities of cutaneous pressure and pain. Alrutz' evidence on the insensitivity of pressure spots to tickle he regards as insufficient, and explains the 'transitional' forms between tickle and itch as cases of admixture of the two qualities.

(*c*) *Regional Sensitivity*.—The attempt to correlate the peculiar distribution of tickle sensitivity with sensitivity to pain and pressure, or with the richness of nerve-supply, has been made by various writers. Richet declares that the parts most sensitive to tickle, the soles, palms, nasal aud labial mucosa, external passages of the ear, etc., are those richest in the filaments of the "*nerfs tactiles*," while Weber, Bourdon, and others maintain that where the sense of touch is most acute or delicate tickle is feeblest, *e. g.*, on the tips of the tongue, fingers, and nose. Weber, Kiesow and Robinson note the relation of the hairiness of the surface to tickle sensitivity, while Darwin and Hall and Allin seek an explanation in extra-sensory factors,—the infrequency of attack or the vulnerability

[1] Curiously enough, Goldscheider makes these very data the basis for the opposite conclusion, *i. e.*, the association of tickle with the *differentiated* organs of pressure.

[2] *Cf.* W. A. Nagel's Handbuch der Physiologie des Menschen, III, 1903.

of the ticklish areas. The sensitive surfaces enumerated by von Frey, the back, the lines in the palm of the hand, and the folds of skin about the nostrils, are obviously related to secondary pain rather than to tickle.

Résumé and Criticism

The writers quoted belong, roughly speaking, to the descriptive and experimental types. The former are primarily interested in speculations regarding the anomalies in the affective tone of tickle and the genesis of the laughter reflex. The latter, of whom Goldscheider and Alrutz are almost the only representatives, are concerned mainly with the qualitative relationships and psychophysical substrate of tickle, Goldscheider identifying it with weak pressure, Alrutz incorporating it in a secondary pain sense. While many observations have been collected, and much advance has been made over the original obscure irradiation-hypothesis and the tendency to mistake weakness of stimulus for feebleness of sensation, further introspective study of the relation of tickle to pain, pressure and the like is now in order. Between the observations of Alrutz and Goldscheider there are, as we have already noted, unfortunate discrepancies, and Goldscheider's conception of the twofold, tickle-pressure aspect of tactual sensation is too ill-defined and hypothetical for unqualified acceptance. In the literature in general a more or less common failure to distinguish between itch, deep-seated tickle, and tickle from a punctiform and moving stimulus, leads to much confusion and contradiction both as to the properties of tickle themselves and as to their interpretation. A further study of the status of tickle as a sense quality, and of its place in the organic or cutaneous continuum, is requisite before any adequate explanation of the peculiar vividness of the tickle consciousness as a whole can be attempted.

Experimental

(*a*) *Peculiarities of tickle to be explained.* The peculiarities by which superficial tickle is set apart from other sensations of the skin appear in the literature above surveyed as follows.

(1) Qualitative indefiniteness and general intangibility.

(2) Relative weakness of the adequate stimulus, and disappearance of the sensation with intensified stimulation.

(3) Susceptibility to fatigue and "excentric anæsthesia."

(4) Inertia and after-duration of tickle sensations as compared with touch.

(5) Diffusion or irradiation of sensation to outlying areas.

(6) Relation to summation, as indicated by intensification of effect with repeated, intermittent or moving stimulus.

(7) Part played by hairs in the intensification or propagation of the effect.

(8) Anomalies in the regional distribution of sensitivity to tickle, which is apparently non-correspondent with the distribution of areas of greatest tactile sensitivity.

(9) Affective tone of tickle, pleasant or unpleasant, and its explanation. Three remaining peculiarities refer especially, though not exclusively, to the deep seated tickle. These are: the disproportionate reflex response to tickle, individual differences in susceptibility, etc., and the relative importance of the "psychic" as compared with the purely sensory in the general phenomenon of tickling.

(*b*) *Points to be experimentally determined.*

The points most urgently in need of experimental investigation or verification may be formulated as follows.

(1) Is the qualitative contents of tickle in any given instance homogeneous, or a complex of unlike components? Does the peculiarity of the experience reside in the sensation itself, or is it to be referred to the general reflex response, as suggested by Bain?

(2) Are the nerve endings concerned those of pressure, pain, the circulatory system, or some new set; and what is the adequate stimulus intensity? Secondary problems arising here relate to (*a*) the distribution of tickle points, and (*b*) the qualitative affinities of tickle.

(3) Is tickle excited directly, reflexly or by central irradiation? Is it simultaneous with touch or delayed?

(4) What is the relationship of superficial and deep-seated tickle; are they qualitatively homogeneous or dissimilar?

(5) What is the distribution of ticklish areas?

(*c*) *Methods of investigation.*

The following series of experiments was carried on in the laboratory in connection with those on pressure, and with the same observers.

I. Qualitative analysis of tickle, and determination of the adequate stimulus for punctiform stimulation. Repeated introspective analysis and comparison of different cases of tickle were undertaken in order to determine the specific quality of the sensation, the possible presence of two or more components (weak pain, pressure, or temperature), and the similarity or difference of the punctiform and moving varieties. A number of areas on the hand, back, arm, and face were examined for the determination of possible qualitative differences. Trial was made of many different modes of stimulation: punctiform stimulation with hairs of varying radii and length, with glass hairs, with a needle; stimulation with a cork point and a camel's hair

brush; successive stimulation with a moving wisp of paper, brush, or bit of cotton; the twitching of the cutaneous hairs; intermittent stimulation with a tuning fork or a bristle attached to a fork; and lastly, the application of a weak faradic current through a blunt or needle-pointed electrode. Paired observations were frequently taken for the comparison of tickle with pain, touch, and contact sensations proper.

II. Mapping for tickle points. The regions especially investigated were areas 1 cm. or 1/2 cm. square on the palm, back of hand, volar surface of upper and lower arm, outer surface of lower arm, and between the shoulders. The adequate stimulus was found to be a light quick touch with needle, cork point, glass hair, or horse hair. The last proved most serviceable; and a hair set in the von Frey æsthesiometer, variable in length from 2-6 cm., approximately 0.2 mm. in diameter, with a grading of 2.5-8.5 gr/mm. was finally adopted. The conditions most effective for tickle seemed to be those favorable to the setting up of a slight oscillatory or vibratory movement in the skin or underlying parts, with as little liability as possible to damping-off by continuous or heavy pressure. The quick snapping back of hairs was particularly effective, while a slow to and fro movement gave only a sense of touch or pressure. The region immediately about the hairs was found the most sensitive; but, as in the case of granular pressure (see Section A), a small group of points rather than a single point of maximal sensitivity was discovered. This area was rather larger for tickle, corresponding roughly to a circle at whose centre the hair emerges from the skin, whereas pressure sensitivity is usually confined to a small segment to the windward of the hair. On a shaved area, however, the best tickle and the best pressure are usually obtained from the stimulation of the same point, the stump of the hair.

III. Determination of the place of tickle in graded intensity series. This experiment was undertaken in order to determine the place of tickle in the intensity series, Goldscheider maintaining and Alrutz denying that it is the first sensation to emerge above the tactual limen. The spots of greatest sensitivity to pressure or contact were located, usually in the close vicinity of the hairs. Each spot was then stimulated with a series of graded intensities; the stimulus hair above described was employed. Qualitative or descriptive judgments were given on each member of the series. As noted in Section A, tickle usually appeared in the lower part of the series but not always at the foot. A stimulus intensity of from 2.5-4 mg/mm. was finally chosen as the most effective for tickle.

IV. Testing and mapping for tickle with variation of the

conditions. Etherized, cocainized, and fatigued surfaces were examined, usually with a moving stimulus, in the hope of discovering a disassociation between tickle and touch or tickle and pain. So far as identifying tickle with either pain or contact was concerned, the results were negative. The etherized surfaces showed a loss of brightness for moving tickle and a tendency toward the substitution of a lasting itching quality (probably reflexly excited) for the usual superficial and quickly fading tickle. There was also a dulling or disappearance of contact sensations as distinct from pressure, and of superficial pain, but apparently not of the pain from deeper-lying endings. A scalded surface explored with a light moving stimulus gave all gradations from a sharp tickle to vivid but quickly passing pain, between the normal margin and the point of severest injury.

In connection with the use of cocaine, the discovery was accidentally made that oiling of the skin brings about a reduction of its sensitivity to tickle. Careful mapping of an oiled area indicates that this is less a case of lowering of sensitivity than of the sharper delimitation of the maximal sensitivity spot to the vicinity of the hair. The explanation is apparently this: the reduction of the elasticity of the skin through oiling disturbs the conditions favorable to the propagation or continuance of the slight vibratory movement usually set up by the light setting down and removal of the stimulus hair.

V. Testing pain and contact points for tickle. The intermediate bright points noted in Section A were tested for tickle with punctiform and intermittent stimulus (bristle on a vibrating tuning fork), mostly with negative results.

VI. Regional sensitivity to tickle. Comparisons were made of adjoining and widely separated areas. The palms of the hands were found to be peculiarly sensitive to punctiform stimulation, the tickle there tending to pass into itch. There is a diminution of sensitivity[1] from the wrist to the shoulder, and the radial and ulnar margins are apparently more sensitive than the intermediate surfaces, and the volar than the dorsal surfaces of the arm. The lips yield a peculiarly poignant tickle throughout a wide range of intensities; the tongue only with the finest of hairs or, better, with a vibrating fork.

(*d*) *General Results*

1. Descriptive. With punctiform stimulation, tickle appears

[1] 'Sensitivity' as here used refers not to the stimulus limen (which really varies little) but to the vividness of response as tested by a considerable range of intensities, the more insensitive areas refusing to yield a lively tickle except with moving stimulus.

in two forms, of which the first occurs in the vicinity of hair bulbs, on areas such as the arm. It usually comes out immediately, but sometimes only after a brief delay, and is hazily localized on the surface or just below it. It is characterized as "a quick kind of contact," rising abruptly and dying away slowly into a long, cloudy after-image, and is sharp or bright in comparison with an ordinary pressure. It is distinguished from contact by its abrupt rise; its moving, creeping character, which apparently causes it to be felt as a line rather than a point; its indefiniteness; and its liveliness, which is usually accompanied by the desire to rub it away. While tickle ordinarily seems to bear no resemblance to dull or even granular pressure, the observer believes that in quality it is hardly distinguishable from contact or light touch, a weak phase of which is often characterized as "just about to be tickle."

The observers generally agree that the judgment "contact" is constructive, associative, or objective rather than descriptive. It refers to consciousness of area, of locality, or of the nature of the stimulus (which is usually visually imaged), rather than to mere sensation quality, which often eludes recall, and is apparently immediately swamped in the objective judgment. The tickle judgment, on the other hand, usually attends an absence of objective reference, conditioned not so much by the mere quality or faintness of the contents as by its unsteadiness or flickering. *E. g.*, the weakest contact sensation is not necessarily the ticklish one; even with the utmost care and uniformity in the setting down of the stimulus-hair, tickle is not invariably associated with the weakest stimulation, and contact may appear below tickle in the graded intensity series. Further, the tickle judgment seems as much dependent on the sharpness and uneasiness of the immediate contents as upon the reflex shudder, desire to rub or other response which it excites; the tickle judgment is frequently definite when the tendency to such response is practically nil.

The second type of tickle, which perhaps differs from the first only in the matter of intensity and the amount of diffusion of the stimulation, is obtained most characteristically from the lips or the palms of the hands, and is described as quick, sharp, lively, and akin to pain, thrill-like or vibratory, tolerably well localized, with considerable irradiation and after-image. It also is sensed without delay, and the tendency to rub it away, which sometimes appears, seems to be associated mainly with the itching after-image which succeeds.

The tickle accompanying or following a moving stimulus resembles this second type, and is practically the same for the palm and for a hairy surface. It is variously described as phantom-like, feathery, diffuse, of a wavy, pulsating character;

after further practice in observation it is characterized as sharp, bright, intermittent, irradiating, almost tingling, suggestive of a weak, intermittent current, and is localized on the skin or just below it. The possible relationship of tickle to pain is indicated in the faint suggestion of soreness accompanying a swiftly moving stimulus, especially after one or two repetitions, and further in the unpleasant sharpness or liveliness which such a tickle shows on an inflamed or tingling surface, where all gradations from tickle into a closely similar but stinging or "nettly" train of sensations may be obtained. It is possible that this semi-painful character is largely due to reflexly excited after-sensations, into which tickle often indistinguishably merges as it approaches itch; in part, however, it seems a function of the "sharpness" of the immediate tactual contents.

In this tickle of the moving stimulus a second wave of sensation often follows at a just noticeable interval. This after-train may be either a wake of sharp popping points, or a cloudy blur, and is frequently fused either with a faint elusive glow, or with successive waves of cold and warmth. It bears a close resemblance to the gooseflesh shiver, dies away into a dull, superficial tension,[1] and suggests the possibility of a vasomotor reflex affecting either tactual or circulatory endings. This secondary wave is sometimes less, sometimes more ticklish than the primary sensation, grading in some cases into itch. With repetition the tickle of the moving stimulus is at first enhanced (possibly in the after-effect mainly), later greatly dulled, becoming mere contact or pressure, steadier, and indifferent in tone. The brightness of the contact quality suffers at the same time, however; only deep pressure remains unimpaired.

In summary, the observers agree that the peculiarity of the tickle experience is not exhausted by reference to the reflex disturbance excited by it, but resides somehow in its immediate sensory contents. The characteristics common to all forms of tickle are brightness or vividness, inconstancy or unsteadiness (which may be a matter of fluctuations of intensity, or of intermittent, scattering, or successive response of different points), and an ill-defined reference to the epidermis.[2] It is also certain that the weakness of intensity usually predicated of tickle is a matter of stimulus interpretation rather than a direct sensation estimate. The intensity of a light ticklish touch can hardly be equated, psychologically speaking, with anything short of pain, certainly not with the dullish pressure sensations aroused by a considerably heavier pressure on the same area.

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[1] *Cf.* Section A.
[2] The presence of temperature sensations is apparently non-essential.

The question whether the brightness or sharpness of tickle is to be regarded as a matter of genuine qualitative tinge, of clear-cut temporal or spatial definition, or as a function of attention, conditioned by the intermittence or flickering common to tickle, has been previously touched upon (in Section A). While there seems to be no sufficient reason for distinguishing tickle from contact or surface pressure as a unique quality, the peculiar vividness of the tickle quality remains somewhat perplexing. There seems some ground for identifying this vividness with clearness or the effectiveness of attention. Certain it is that tickle possesses a strong intrinsic claim upon the attention. Nevertheless, the suggestion that the brightness of tickle is merely a matter of exaggerated clearness and definition in attention, and that any lively intermittent tactual sensation would be ticklish, is unsupported by the facts. Intermittent stimulation with the electric current, tuning fork and the like may or may not be ticklish. Observation goes to show that a medium intensity and a medium amount of analysis are optimal; above and below these points we have either a blur, a complex of sharp points, or dull pressure. Whenever the vivacity of the tickle response is dulled by rubbing, fatigue, or the use of weak intensities, when (with electric stimulation) the individual points of sensation are too weak or too rapid for analysis, and when they are too intensive for integration and each in turn stands out in attention, the sense of tickling vanishes. Further observations bearing on this point are as follows. Tickle is most ticklish when just out of the focus of attention, or rather when the whole mass of sensation rather than any one point of it is the object of attention. The quick, tremulous, ticklish contact-sensation from punctiform stimulation of the palm becomes, with a slightly different setting or focussing of the attention, mere indifferent contact. Again, in a long observation period the observer may lose the tickle judgment entirely, apparently as the result of increasing skill and practice in analysis and objective reference.[1]

With regard to the relation of tickle and itch, the observers generally agree in distinguishing the latter as more persistent, painful and intolerable. Its fluctuations in intensity are also more marked.

2. *Distribution of tickle points with reference to the end-organs of pressure.* With punctiform stimulus, the palm alone seems to yield tickle from any point of its surface. Areas on the back of the hand or arm give tickle only in the immediate vicinity

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[1] This disappearance might of course be traced to the wearing-off of reflex excitability, but for reasons which will be clearer later we believe that this is not the whole explanation of the matter.

of hair bulbs. In the intermediate spaces, contact or nothing is felt with the same stimulation. When the hairs are closely shaved, some observers hesitate to give the judgment of tickle except when the stump of the hair itself is touched.

The relation of tickle and pressure points is hardly ambiguous. Tickle may be obtained from any pressure spot if trial is made of different stimulus intensities; from the intermediate pain points very doubtfully even with intermittent stimulation.[1] Further, the points of maximal sensitivity for tickle and pressure agree fairly well, in view of the fact that the position of neither seems to be absolutely fixed,—sensitivity being confined to a minute semi-circle rather than to a point, and shifting with partial fatigue. Alrutz' observations to the contrary, as has been already pointed out, are probably due to the adoption of itch or secondary pain phenomena as the standard of tickle.

3. Regional sensitivity. While there are wide variations in vividness, and in the certainty with which the judgment of tickle is rendered, most areas yield tickle with either punctiform or moving stimulation. No absolutely insensitive area, so far as we have investigated, exists, with the exception of certain highly specialized surfaces such as the cornea. Both the tongue and the finger-tips, usually listed as non-ticklish, are found distractingly so if tested with a stimulus of the right intensity, moving or vibratory. What has been called insensitivity seems to be merely a matter of fewness of hairs, infrequency of end-organs, thickness of skin, or position of limen.

With a moving stimulus the following areas gave ordinarily the most vivid tickle: the roof of the mouth; the folds of the skin behind the ears, below the eyes, and between the fingers; the inside of the arm (which was increasingly sensitive toward the wrist); the palm of the hand; and the lips. These surfaces, it may be observed, are characterized either by abundance of fine hairs, or thinness of skin, or the presence of corrugations which may further irregularity and intermittence of sensation. For punctiform stimulation the favorable areas were fewer,—the lips, lower eyelids, cheeks, palm, and outer edge of the forearm giving the surest tickle. These areas apparently have only one feature in common, their richness in Meissner corpuscles or the thickness with which the hairs are set (especially in the case of the cheeks). In general, capillary dilatation seems slightly to increase sensitivity; whatever reduces the elasticity or tension of the skin, as oiling, seems to decrease it.

[1] The possible participation of these intermediate points of sensitivity in the tickle of a moving stimulus is another matter.

(*e*) *Conclusions as to the Histological Substrate of Tickle.*

1. Summary of peculiarities. No good reason is forthcoming for divorcing tickle from the sense of touch or superficial pressure. The practical identity of the points of maximal sensitivity of the two, the appearance of tickle without delay in an enormous number of cases, and the parallel effects of fatigue or over-stimulation on tickle and contact, furnish direct evidence of their close relationship. Indirect evidence of equal weight is furnished by the introspective verification of the qualitative similarity of the two sensations, based upon our analysis and study of granular pressure in Section A.

Tickle is characterized by its intangibility and absence of clear objective reference. The explanation of this peculiarity must be sought elsewhere than in its nearness to the limen, and its consequent qualitative indefiniteness and inability to call up assimilatory ideas. Tickle is not invariably correlated with the feeblest tactual impression; the latter is often sensed as contact, while tickle appears only at a higher point in the intensity scale. Further factors to be accounted for in any theory are the vividness, unsteadiness, and apparent diffusiveness or irradiating character of the simplest punctiform tickle.

2. Hypothetical. The inconstant wavering character of moving tickle, and the sense of motion which accompanies even the weakest tickle arising from punctiform stimulation, suggest that the uniqueness of tickle lies less in any qualitative peculiarity than in the irregular temporal course of the sensation, and its consequent grip upon the attention. The physiological conditions of this intermittence or unsteadiness must then be sought. In the case of the tickle attending a moving stimulus, the swift appearance and disappearance of successive points of sensation, combined on hairy areas with the effect of the vibratory response of the elastic hairs, may account fairly well for the phenomenon. Again, in the case of the ticklish halo surrounding a core of contact or coming out after the removal of the stimulus, explanation may have recourse to the irregular response or ringing off of different endings. The spread of stimulation to adjacent end-organs is especially possible with a light quick stimulus, which may form the starting point of a series of tiny ripples.

The query concerning the minute tickle, unattended by contact, which proceeds from punctiform stimulation of the skin, must next be answered. Does this also involve the response of more than one nerve-ending or nerve organ? In the case ot the palm, where the tactile corpuscles are thickly set, it is doubtful whether with the utmost caution and the use of the finest point the isolated stimulation of a pressure organ is pos-

sible: multiple response may account, in part at least, for the vivid tickle which results. The tickle following punctiform stimulation of a hair-bulb on the arm, when the skin has been oiled and softened to prevent the transmission of the stimulation to the adjacent endings, obviously represents a case for which some other explanatory factor must be sought.

Either the actual physical vibration of the tissues, or some as yet unexplained oscillatory condition within the end-organ itself, or an intermittent internal stimulation attending direct or reflexly excited alterations of the pressure conditions within the capillaries of the skin, would here serve the purposes of explanation. It is worthy of note, in this connection, that an intrinsically oscillatory character was attributed by Meissner to his "contact" sense, and later claimed by von Frey for the sense of pressure. Goldscheider's similar view (propounded in his earlier monograph) we have already noted: intermittence is a normal function of the conditions within the end-organ, especially with light stimulation; a faint stimulus produces a whole series of excitations or vibrations, which are either damped off or transformed to an uniform steady displacement when a heavier continuous pressure is substituted for a light touch. The fact, however, that tickle is not invariably the outcome of the weakest touch stimulation, that for any pressure organ the adequate tickle stimulus is sometimes liminal, sometimes decidedly supraliminal, shows the need of a more careful examination of the conditions.

Our study of the granular pressure sensation furnishes us with a certain cue. If a more or less intensive stimulation of a single pressure organ may give rise to a pressure sensation not in itself quite simple, it is highly probable that the tickle produced by a slight disturbance of the same end-organ will be also in a fashion complex. Indeed, our conclusion was that both tickle and the sensory response to a weak intermittent current represent disintegrations of the 'granular pressure sensation,' which only render its compound structure more obvious. Accepting the Meissner corpuscles and the hair follicles as the end-organs of touch (and of tickle), we have in the number of endings furnished to a single organ and the possibility of their successive or asynchronous excitation an immediate histological basis for irregularity or unsteadiness of sensation. Further, the fact that the irregularity of response from these endings, which is sensed as tickle, is the customary but not the invariable result of light stimulation is readily intelligible. The stimulus may be insufficient (in area or intensity) to produce the general disturbance of the endings which gives rise to tickle; the fluctuations may be below the flicker limen. Or, when a penetrating or sharply pointed stimulus is applied above a hair

follicle, it is conceivable that one or more endings only are affected directly, and that little commotion is produced in the organ as a whole; the resulting sensation would then be identified as contact.

This asynchronous response may also be made to account for the apparent irradiation or diffusion of a simple tickle sensation (apart from the cases of actual gooseflesh response). The unsteadiness of the tickle of punctiform stimulation is easily confused with the intermittence of sensation which is ordinarily associated with the movement of an object along the surface of the skin. Since an intermittent stationary stimulus is rare in experience, and the difference in local sign for minute distances is slight, an interpretation in terms of movement frequently follows tickle, and an illusory judgment of irradiation or diffusion is given.

The origin of the tickle after-image, which is sometimes a mere breath-like phantom, sometimes vivid and bordering on itch, remains to be considered. With the exception of the possible addition of a painful factor in itch, it is in all ways similar to the primary tactual tickle just described. Whether it is to be traced to circulatory endings, to indirect excitation of the end-organ of touch through reflexly aroused vaso- or pilo-motor changes, or to mere inertia of the original tickle sensation, must remain a matter for speculation.

Although the above suggestions as to the mechanism by which a simple touch may be given an oscillatory character are impossible of verification, the fact that intermittence or irregularity, however produced, is an essential in tickle finds sufficient evidence in our introspections. The significance of this factor in the explanation of the tickle consciousness as a whole is further apparent in the supplementary introspections on ticklishness and tickling which follow.

(*f*) *Supplementary Introspections*

Deep-seated tickle. The following casual observations on deep-seated tickle go to show that here also a just perceptible unsteadiness or intermittence is the important feature. If a light swift friction of the surface of the arm is repeated until the bright superficial tickle has worn off, a deeper, dullish, unsteady and uncomfortable feeling succeeds, accompanied by an almost irresistible impulse to shudder or draw away. If a ridged surface, such as the finger-tip or the sole of the foot, is similarly treated, the effect is even more striking. The deeper tickle produced by heavy chest massage is similar, of an uncanny and uneasy character, and seems to represent not a solid mass of sensation but a loosely woven tissue in which the individual elements are barely distinguishable. In general, the

deeper tickle is dull as compared with the more superficial; at the same time it contains a certain element of soreness or sharpness. This deeper tickle is closely similar to the vaguely ticklish after-image which sometimes follows the heavy or continued stimulation of a single hair-bulb. The latter sensation is dull and of a vague uncertain character, due apparently to the unsteadiness which is a common feature of all tickle.

The peculiar liveliness of the reflexes excited by deep-seated tickle is probably due, not only to the customary exaggeration of dynamogenic values by discontinuous stimulation, but also to the greater number of nerve endings affected by massive kneading or rubbing. The discontinuity or unsteadiness is rendered more obvious by successive intensifications of the sensation when the stimulus passes over the ribs or the ridges on the soles of the feet or the palms of the hands.

Affective Reaction and Reflexes. The pleasantness and unpleasantness of tickle seem to be less a function of its immediate sensory aspect than of the muscle response which it excites and the suggestions aroused by the stimulus, and are largely dependent on the mental or nervous state of the individual. The pleasantness of the tickle in the ribs may be, in part at least, merely a suggestion from the smiling set of the facial muscles reflexly excited along with the more general muscle contractions; the pleasantness of a more superficial tickle is probably due to its light touch component and the soothing organic response and general relaxation conditioned by it. The unpleasantness of deep-seated tickle can usually be traced to the violent contraction of the diaphragm and adjacent muscles and the accompanying compression of the internal organs. The unpleasantness of cutaneous tickle is probably due partly to the sharpness of the sensations, which are almost akin to pain, largely to the vague, uneasy suggestion of the unknown, which gives an uncanny tinge to tickle as to the gooseflesh shiver. On the whole, the subjective moment in the immediate sensation of tickle is best described neither as pleasantness nor as unpleasantness, but as excitement. The significance of this statement we shall examine later.

(*g*) *Final definition of tickle.* We have cited already various facts which indicate that the definition of tickle is not complete when its constituent elements have been described and classified and their intermittence emphasized. Tickle is something more than a complex of intermittent or oscillatory sensations. The fact that intermittent stimulation with tuning fork, interrupted current, or moving touch is not always ticklish indicates that tickle is a function of certain rates and intensities only. Too slow a rate and too high an intensity are alike unfavorable, and apparently for the same reason; they

facilitate analysis, and the complex tends to fall apart into individual points, each capable of definite localization and objective reference. Further, the sense of tickle comes out most strongly when the initiatory sensations are, so to speak, just in indirect vision, and the attention is dispersed over the immediate sense-contents and the general bodily reaction. When a single sensory component or an assimilatory idea gets the focus of attention, and consciousness becomes analytical or critical, the tickle judgment vanishes. While the sense of tickle is heightened by the direction of the attention away from the idea of the stimulus to the sense-contents, the judgment of tickle is instable, often disappearing at a certain stage of practice in the analysis of this contents. Tickle is then replaced by the perception of contact.

This instability of the tickle judgment indicates that tickle is better defined as a complex of intermittent sensations *at a certain point of fusion or amalgamation*. The favorable conditions are those in which the tendencies to analysis and amalgamation are equally balanced, and we have neither a disconnected series of sensations nor a smooth union. Below the point of adequate fusion, which is dependent upon the rapidity and intensity of the component sensations and on the direction and skillfulness of the attention, we have disconnected sensations, either indifferent or verging on the painful, and capable of individual projection; above it we have complete fusion or steadiness, interpreted as contact and given objective reference, *i. e.*, a perception. —— This conclusion finds corroboration in the following introspective data. The two criteria, aside from the affective tone and tendency to reflex withdrawal, by which the observer finally came to distinguish between tickle and the weakest contact, were as follows: (*a*) lack of steadiness, smoothness, unity, and definite punctiform localization; and (*b*) absence of reference to a stimulus, and of visualization of a definite surface, or of the descent of the stimulus.

The significance of the structural pattern above assigned to tickle becomes fully apparent only when the tickle consciousness is examined as a whole, in relation to experience in general and in comparison with other typical complexes, perception in particular. In so examining it we come first upon certain characteristics to which so far we have done scant justice, its vividness, its characteristic feeling-tone, and its power to excite a general muscular reaction. The latter feature, which has given rise to so much speculation is, no doubt, in part to be explained by reference to a familiar physiological fact, the greater effectiveness of intermittent stimulation in exciting reflex centres to action. Further, this motor reaction is, to a certain extent, self-propagating, since certain of the sensations

to which it gives rise, especially those arising from the goose-flesh shiver, and the activity of the plain muscle fibres in the skin, supplement the tickle sensations proper and prolong the motive to general muscular reaction. The reinforcement which the motor reaction thus lends to the tickle sensations proper is probably part of the secret of the hold which tickle has upon the attention. Physiologically and psychologically, perhaps, we might say that the motor accompaniment of tickle and the hold of the latter upon the attention are both functions of the unsteady, fluctuating character of its components; biologically, they may be interpreted as expressions of the significance of movement in the earlier phases of the racial history. The motor reaction in general may be regarded as representing a survival either of specific reflexes associated with the primitive protective functions of the once all-important sense of contact, or of a primitive type of neural mechanism in general, in which excitations were guided directly into channels of motor discharge rather than of perceptual functioning.

This motor reaction may, however, be further interpreted as standing in a certain fixed relation of dependence to the peculiar structural pattern and degree of amalgamation of the contents which is known as tickle. This is indicated in the facts above cited, that the motor and organic reaction disappears, along with the observer's sense of tickle, when consciousness becomes critical, analytic and perceptual. This circumstance, which might be set up as an evidence of the dependence of the sense of tickle on motor reaction, we should interpret as follows. So long as attention is incompetent to cope with the semi-amalgamated series of impressions which constitutes tickle, this contents functions as a whole, the primitive sensorimotor path of least resistance is followed, and we get the reflex shudder and incipient innervation of the appropriate muscles of withdrawal or laughter, effective in proportion to the cumulative force of the stimuli and the motor excitability of the individual. When, however, analysis of the contents into its individual components is effected, the energy otherwise overflowing directly into motor channels is expended in the excitation of the sensory or ideational centres, and the assimilatory ideas and images, visual, verbal or motor, push the immediate sense-contents out of the centre of consciousness, the muscular reaction is largely effaced, and the 'perception' of certain definite sensations of contact succeeds the 'feeling' of tickle. Tickle is tickle only so long as it can monopolize the field of attention, in the form of an unsteady mass of sensation with which analysis is unable fully to cope.

'Feeling,' in the sense in which it is here employed, denotes, of course, neither an ultimate affective element, nor the un-

analyzable and unlocalizable, in the usage of Kroner, Lagerborg and others. As here used it refers merely to that peculiar type of consciousness which, in terms of function and cognition, represents not so much the abstract physiological properties of the process experienced as its physiological effect on the organism as a whole. In structural terms, it is simply that mass of ill-localized, ill-analyzed sensation which, because of its cohesion and its persistence, exercises an intrinsic claim upon the attention, and is distinguished by its peculiar vividness and its strong dynamogenic value. Tickle, as we have already said, while never actually indifferent, is best described neither as pleasant nor as unpleasant, but as exciting. In so speaking we have no reference to a third affective element; the tickle experience is, in fact, not so much exciting as it is itself (a certain form of) the 'feeling' of excitement.

It is in this connection especially that the true relation of tickle to organic sensation becomes apparent. The classification of tickle with the *Gemeinempfindungen* by Weber, Wundt and others has already been noted. While we have decided against the strictly organic character of tickle, its status as 'feeling' at once allies it to the internal sensations, since these customarily enter into feeling-combinations rather than into objective perceptions. From such organic complexes as hunger, disgust, excitement, and the like, tickle differs mainly in its greater accessibility to analysis, and hence in its greater tendency to disintegration.

While the basis of our classification of tickle with the bodily feelings is not precisely that of the older writers, the difference lies mainly in greater explicitness and in the attempt to formulate the difference in more distinctively psychological terms. The old division of experience into the objective and subjective was philosophical and schematic rather than psychological. In Weber's terminology, the use of the terms unprojected and projected sensation is strongly suggestive of the distinction between mere sensation and intuition or concept in the system of Kant. The peculiarity of the latter is not a matter simply of the addition of centrally excited ideas, assimilative or associative, but rather of some mysterious transformation which the contents undergoes at the hands of the understanding. The fact that in perception sensations are referred to the external world, in feeling to oneself, contains in itself no *a priori* reason why they should be differently sensed. Again, Wundt's reference to the external or internal character of the stimulus, and the lack of exact correlation between the course of stimulus and sensation when the stimulus is internal, expresses only very remotely the actual basis of the differentiation of contents. The internal origin of a sensation is undoubtedly a drawback

to its clear perception, mainly on account of the limited opportunity it offers for the simultaneous experiencing of the same physical fact through the medium of other senses, and the consequent failure to set up associations which might serve as an aid both in analyzing it out from other contents and in orientating it as a perception in consciousness. Tickle, however, fails of analysis and projection not from the lack of external associations at its command, but because of a lack of unity, and a baffling swiftness and irregularity with which the processes of perception cannot keep pace. In addition, the kinæsthetic or organic sensations to which it gives rise tend to assimilate it to themselves, and thus weaken the motive to analysis and bind it the more firmly to the subjective.

The lack of objective reference is, then, rather an effect than a condition of the peculiar character of tickle as a 'feeling;' a condition merely in so far as it means the non-appearance of certain centrally excited sensations which would help to break down the tickle complex; an effect, in that the temporal, spatial, and intensive irregularities which constitute tickle a semi-cohesion of shifting sensations, with the status and magnified motor value of a 'feeling', are precisely the factors which stand in the way of its objective reference or projection.

Tickle may thus be finally defined as an intensely vivid complex of unsteady, ill-localized and ill-analyzed sensation, with attention distributed over the immediate sensory contents and the concomitant sensations reflexly evoked. Its immediate sensory contents is not qualitatively different from contact, but in actual experience tickle is distinguishable from the ordinary contact complex in its character of a 'feeling' rather than a perception.

A NEGLECTED MEASURE OF FATIGUE

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Von Kries (1) seems to have been the first to have recognized the psychological implications of the maximum rate of repeated voluntary movements. His experiments were made by attaching an electric wire to the end of the fingers, which closed a contact when the finger tapped upon a metal plate. The experiments were not especially systematic, and maximum rate only appears to have been taken into account. We find, however, a distinct recognition of the neural character of the limit placed upon this maximum rate, and of its relationship to that of the incomplete tetanus in sustained muscular contractions, as also observed by von Kries, previously by Horsley and Schaefer (2), and by Schaefer, Canney and Tunstall (3). Two years later Griffiths (4) repeated and elaborated these experiments, with especial reference to tetanus of the loaded muscle. "There is a gradual increase in the number of muscular responses per second as the weight is increased up to a certain number; any increase in the stretching weight beyond this point is accompanied by a decrease in the number of muscular responses per second." His most important observation for us, however, was that this rate in tetanic contraction usually increases slowly until about the end of the first minute, and then decreases slowly. The fatigue phenomena, however, are very slight for the longest period observed, 2½ minutes. It is necessary to bear in mind this result for tetanic contraction for comparison with the much more marked fatigue phenomena obtained when, instead of sustained contractions we are observing repeated voluntary movements. The observed relationships between these rhythmic discharges in tonic contractions and the most rapid repeated voluntary movements, as well as clonic contractions and tremors, I hope to discuss subsequently in a separate paper. For the present we may confine ourselves to the most rapid repeated voluntary movements, which have since had considerable experimental development under the name of the "tapping test."

Although the first psychological observation is the work of von Kries, the later development of the test has gone on almost wholly outside of Germany. It is practically unknown to the

Psychologische Arbeiten, that home of generalized researches, especially with reference to continued work. In 1891 Dresslar (5) took up the matter independently, publishing the results of an extensive series of experiments upon himself, as well as minor observations on other subjects. A mechanical form of counter was used, the time being taken for 300 taps. The principle of the instrument, while giving no account of the individual taps, might register gross fluctuations in rate. The results, however, speak very poorly for its trustworthiness in this respect. It seems to have given no indication of the extent of fatigue or appreciation of its significance. Only the slightest evidence of "incipient" fatigue was noted, whereas later investigators have repeatedly found the fatigue losses to be very marked even within the first 100 or 200 taps. The subject worked at the limit of practice, and it is interesting to note that "it was thought that after practice this number (300) would not be fatiguing," whereas actual observation shows that fatigue loss may be even greater at the limit of practice than at its beginning. All sensations of fatigue, Dresslar notes, had ceased with the first few days of the work. This is in accord with the writer's observations; the sensations of fatigue disappear with continued practice, but the objective phase persists. The average rate at the limit of practice for 300 taps was 8.5 per second, which is abnormally high. The work was done with the right hand; a few experiments with the left hand gave an average rate of 5.3 per second. This places the right and left hands very much further apart than they appear in the studies of Bryan, Marsh, or the writer. It can hardly be taken as other than an indication that the practice of one member in this function does not essentially affect the performance of the other members; so far as I know it is the most important single datum that we have on this point. The rate was found to be decreased by physical, and rather increased by mental work; some unsystematic observations of the writer are in accord with this result. How much confidence can be placed in the daily rhythm records it is difficult to say, because a performance in the tapping test at the limit of practice exerts a "warming up" influence that extends over a considerable period. However, the writer's experience would entirely confirm Dresslar in saying that lack of practice would account for the failure of such rhythms to develop in his two other subjects. The analysis of the practice of these subjects, however, is almost certainly at fault, for practice improvement is by no means eliminated on the third day, and, as has been said, gives little or no immunity to fatigue losses.

Presumably because of its relative simplicity as well as its high statistical reliability, we next find the test pitched upon

as a measure of "voluntary motor ability" in two investigations of mainly educational bearing. The first of these is the well known research of Bryan (6). The apparatus here used is a form of mechanical counter, registering the number of taps made during a standard interval of time, uniformly five seconds. While the method can of course give only a very incomplete idea of the actual fatigue phenomena, it seems to have demonstrated them far more reliably than the instrument used by Dresslar. A lowering of the rate was observed after 10 or 15 seconds work; it actually begins somewhat sooner, but the method would hardly detect it. We have no precise data as to what happens after the first minute or less; Bryan seems to have found that this decrease goes on at a uniform rate for about 10 minutes, when it becomes slower but continues until a zero is reached after some hours. He notes the question of partial recovery from ordinary fatigue: in the writer's experiment it may be taken as absolute in less than three minutes of rest from thirty seconds tapping. The main body of the work, however, is concerned with the increase in gross rate as related to age,[1] and beside having nothing directly to do with fatigue phenomena, is too familiar to need recapitulation here; the main objection to it is that it hardly recognizes sufficiently the effects of either practice or warming up.

In the work of Gilbert (7) we have the first definite use of the test as a fatigue measure. The subject executed taps on a telegraph key for 45 seconds, the first and last five seconds being recorded. As a fatigue measure, Gilbert gives the per cent. of loss in rapidity during the last five as compared to the first five seconds. The susceptibility to fatigue as indicated in this measure decreases uniformly for both sexes with increase in age, the extremes for each sex being about 21% at six years and 14% at seventeen years. It is noteworthy that the boys show throughout a greater percentage of fatigue loss than the girls, though their initial superiority is sufficient to leave the balance still in their favor. Havelock Ellis has cited this result as an example of the "more continuous character of woman's activity," but there would seem to be a possibility that in the class of subjects used a motor measure of this sort would interest the boys more than the girls, and they would consequently try harder, and tire quicker.[2] In this connection it may be observed that the adult men used as subjects by the writer show on the whole about as much fatigue loss during

[1] See also, with reference to the speed of the different joints, Woodworth, The Accuracy of Voluntary Movement, Mon. Supp. 3, pp. 108-10.

[2] See, however, the results of Bolton, quoted below.

30 seconds as Gilbert's children did during 45, which may argue for inferior co-operation in the children as a group.

Gilbert (8) also reports a subsequent similar series of experiments, made upon school children in Iowa, the first having dealt with Connecticut children. A mechanical counter was used. The general results are similar, though there are some special differences of interest. As to the number of taps in 5 seconds, the Iowa boys begin at 22.1 at 6 and do 34.4 at 17, while the Connecticut boys do 21.0 and 35.0 at these ages. For the girls the figures are 22.3 and 33.8, 19.7 and 31.5 respectively. During the earlier years the Iowa girls are slightly superior, while the Connecticut girls were always inferior to the boys. An interesting aspect of the results is found in the classifying of the children by their teachers into 3 groups, bright, average, and dull. In gross rate, the bright subjects are about equal to the average subjects, the dull subjects somewhat inferior. As regards fatigue phenomena, the girls again uniformly lose less than the boys, though the difference is practically nil during the last five years, 15-19. As regards the differences in the rating of the children, the results are rather striking. The bright children lose more than either of the others at 6, and less than either of the others at 19; this progressive immunity to fatigue is rather less marked in the average, and least of all in the dull children.

The tapping test also figures among the experiments made by Gilbert and Patrick (9) upon three individuals every 6 hours during a 90 hour sleep-fast. The graphic method was used, the subject tapping for 60 seconds; only the first and last five seconds, however, were counted. No account is taken of practice, which probably obscures most the results somewhat. The rates show little progressive change, but vary more than ought to be expected. The per cent. of loss by fatigue is irregular in the second subject, rather decreases in the first, and increases in the third. It should be noted that the scores of the tapping performances of the first subject are more regular than those of the other two and they may be more trustworthy, since he had probably had more practice with the test. The time of isolated individual taps has also been measured by Seashore (10), and compared with simple reaction time to various types of stimulation.

Contemporaneously with the first work of Gilbert, and also in the same laboratory, Bliss (11) introduced a great improvement in the method, though it involves complications which would render it unavailable save in laboratory practice. Bliss directed attention mainly to the variability of the individual taps, which he seems to be the first to have studied. The method was, of course, graphic, the time intervals being regis-

tered by a magnet in circuit with a 100 v. d. fork. The taps were recorded by the passing of a high tension spark from the tip of the recording point to the drum. It is unfortunate that with such an improved method available more experiments were not performed; the records, each of 180 taps, are only six in number, and all but one are on the same individual. The averages of the records show unmistakable fatigue losses, though I am inclined to think the fluctuations in rate are greater than would ordinarily be found; *i. e.*, the decrease in rate would be more regular. Bliss also speaks of a decided warming up in the rate of the individual taps during the first second; this also appeared in the subsequent experiments of Moore, and while the method used by the writer does not lend itself to very accurate determinations on this point, this initial warming up appears also to be generally present in his experiments. Fatigue losses begin roughly after about five seconds. His essential point, that the variability of the taps decreases as fatigue establishes itself, the results seem to indicate with some assurance.

Moore (12) subsequently used the same method of recording as Bliss, but so modified the method of making the taps that his results are not strictly comparable with those of the other investigators. The movement was confined to the index finger which shifted a slide 5 mm. forward and back as rapidly as possible. So far as the initial warming up in rate is concerned they agree with Bliss, but fatigue, which again appears remarkably late, seems to increase the variability of the taps. The gross rate is relatively low. These series were each about 480 taps long, which the investigator considers very near the point of complete exhaustion. When it is remembered that the freehand tap can be continued at maximum rate for an hour or more, it is quite apparent that the fatigue phenomena of these results are in no way comparable to those obtained by the previous investigators.

In studying practice-transference in this function, Davis (13) reverts to the tap upon a telegraph key, recording the number of taps per five seconds upon a mechanical counter. The results are very peculiar. The gross rates are exceedingly slow; the slowest individual among some thirty normal and pathological subjects tested by the writer would come at almost the average of Davis's subjects. The right toe was the member practiced, as it was endeavored to determine the influence of this practice upon the hands and left foot. The member practiced did not always improve, nor when it did was its improvement always greater than that of the unpracticed members. Upon one subject is recorded an experiment of 900 taps without fatigue loss. Altogether the findings are so out of key with practically the entire remaining literature of the method that it is very difficult to judge of their relation to it.

Binet and Courtier (14), and, more recently, Raif (15), have called attention to the relation which the tapping test bears to facility in playing the piano. The former investigators used the graphic method with air transmissions, giving some idea of the force of the taps. Rates of 6 to 10 per second were obtained for single fingers, the difference between those practiced and unpracticed on the piano being not so much in the gross rate as in the regularity of the force employed. Raif also finds the educated steadier, rather than faster, than the novices, and seems to make an *a priori* assumption that the educated are faster than the ignorant.[1]

Binet and Vaschide (16) introduced a considerable modification, which might at first sight seem to be also a considerable improvement, into the method. They object to the telegraph or other key as affording too incomplete an analysis of the movements involved. Instead, a Mosso ergograph was modified into a sort of myograph, using a weight of 1 kg. to be lifted and released as rapidly as possible. This isolates the movement much as in Moore's experiment, and also greatly increases the muscular effort. But as von Kries has pointed out, the significance of the experiment is essentially dependent upon having as free a movement as possible, a condition which his own experiments, perhaps, realize as completely as any have done. Moreover, it must be noted that if we isolate small muscles like those of the finger, and especially if we weight them, we complicate the experiment with a second fatigue factor whose relation to the specific fatigue phenomena of the test it is very difficult to estimate. We fatigue the muscle with reference to the force of its movements as well as to their speed. Various considerations render it desirable that whatever joints we are testing for this function should have as nearly constant muscular power behind them as possible. These conditions are probably best obtained by allowing the subject to select his own preferred method of tapping, and simply to see that he maintains it throughout the experiment. Under ordinary conditions this usually amounts to a combined wrist and elbow tap, with the elbow rested upon the table. No one will dispute that under proper conditions the myograph is ultimately more precise and accurate than the key, but such an instrument would have to be of so delicate a construction as to be very inconvenient for ordinary experimental work, while its advantages as a measure of individual differences in the function would be largely factitious, as Binet and Vaschide themselves admit.

In spite of these essential differences in the isolation and

[1] *Cf.* also Davis's paper, p. 13.

loading of the muscles, their results are not very different from what one would expect from the previous researches. The tests were twenty-five seconds in length in 15 subjects, and the rates varied from 3 to 8 movements per second; from considerably below to rather above what is ordinarily obtained. The average rate at the beginning of the experiment is given as 5 per second, that at the end as 3.5 per second. There is thus a considerable fatigue loss in rate which the investigators state to be regular, though no precise measurements are indicated. Such analyses as are given indicate that the decrease is not regular, but, as natural, most marked at the beginning. The amount of loss is rather more than one would expect, owing presumably to the loading of the muscle, and shows more marked individual differences than are usually found; it is a question whether these do not as much represent differences in co-operation as in ability for the test itself.

A return to the simplicity of von Kries's procedure is found in the method used by Marsh (17). The subject tapped upon a metal plate with a stylus held in the hand, the taps being recorded by an electric counter similar in principle to the Ewald chronoscope used by Gilbert. The unit of the experiment was the time taken to make a specific number, usually 100 taps, as measured with the stop watch. A large number of experiments upon different groups of subjects gave average rates varying for the right hand between 6.7 and 7.5 taps per second, and for the left hand between 5.8 and 7.2 taps per second. These ranges are small because the averages do not represent records of individuals but of groups; the relation between the right and left hand is about the same as that obtained by the writer. It will be remembered that the afternoon records quite generally surpassed those taken in the morning. This investigator also performed an extensive series of experiments on himself, taking the time for 200 taps at different portions of the day. These results are in conformity with the above, but they do not maintain a very close correspondence with the daily rhythm curve of Dresslar; moreover, the later periods of the evening, when Dresslar did not work, are found to be the most rapid of all. No account is taken of fatigue phenomena, but so far as the gross rates are concerned, these experiments probably combine extensiveness and reliability in as good a ratio as any that we have.

There are three more or less general researches of educational bearing that have also made use of the test, among others. Bagley (18) employed a telegraph key and an electric counter, finding no special correlation between tapping rate and class standing. Bolton (19) employed a mechanical counter, in a series of experiments with much the same object, obtaining

rather contrary results. Two groups of children were selected, apparently mainly according to home environment, and submitted to various tests. The groups consisted of children of eight and nine years. Between the right and left hands no significant difference between good and poor was found, but in respect to gross rates, the good at nine differed more from the poor at nine than the good at eight did from the poor at eight. Also did the good at nine differ more from the good at eight than the poor at nine did from poor at eight. The statistics are not altogether satisfactory, but the differences would seem large enough to be significant. and to have the interpretation which Bolton suggests for them. It is also a very suggestive result that in successive trials the good children improved, while the poorer lost slightly; but the phenomenon is one to which I should prefer to apply the term "warming up" rather than practice, which I would reserve for gains of a more permanent character than these seem to be. As regards sex, the girls tap faster than the boys, in direct opposition to the first results of Gilbert. If this difference is significant it is also suggestive that the poor girls are more superior to the poor boys than the good girls are to the good boys.

Kelly (20) improved the evaluation of the test by having the subject tap for 60 seconds, taking the reading every ten seconds. A fairly precise fatigue curve can thus be obtained, but unfortunately the readings cannot be made accurately enough, at least with a subject who taps at all rapidly. This is at present the most objectionable feature of accurate work with the tapping test; the graphic method is necessary, and its evaluation very tedious. Nevertheless there can be but little doubt that should the test demonstrate sufficient special value as a fatigue measure, these difficulties could be obviated through special apparatus.

An interesting observation is reported by W. G. Smith (21), who used the graphic method and air transmission in comparing the rates of normal and epileptic individuals. There is probably no significant difference in the gross rates, whose average is about normal, but while the normal average was 6.3 per second for the first eight seconds and 5.9 for the second eight, in the epileptics it was 6.2 for the first eight and 6.3 for the second eight. The normal individuals fatigue, while at least some of the epileptics must have warmed up considerably during this period. This phenomenon is very rare in normal individuals, but has been observed with some regularity in certain psychoses; its interpretation is by no means clear.

To sum up, the maximum rate of repeated voluntary movements is a function that practically every investigator working with sufficiently accurate methods has found to be subject to

fatigue effects, though the degree of this subjection has differed considerably. Nothing is definitely known regarding the relation of fatigability to the gross rates; *i. e.*, whether faster individuals are likely to fatigue more than slower, nor is the significance of the gross rate itself well understood. Little if anything can be regarded as established in regard to the practice phenomena of the function either in respect to gross rate or susceptibility to fatigue. We have a rough knowledge of the limits within which the initial rates vary, *i. e.*, from about 5 to 14 per second according to the individual; it has not been brought out in what way this rate is correlated with other and deeper mental faculties in the individual, except in so far as is given in Gilbert's and in Bolton's figures. In spite of these considerations, the writer's experience with the test, amounting to some 1,500 individual fatigue curves upon upwards of 30 subjects under many varieties of conditions, seems to justify the belief that we have here an experiment that will in every way bear comparison with such fatigue tests as have found more general employment as measures of this function.

When we speak of a measure of fatigue we may mean either a measure of the state of fatigue or of the susceptibility to fatigue.[1] In measuring experimentally a state of fatigue we usually have certain objectively given or assumed fatigue conditions, and we attempt to determine what has been the effect of these conditions upon some function or functions subject to psychological measurement. Various forms of psychological tests have been used in this way, Griesbach made the suggestion, now rather gone by the board, of cutaneous sensibility; the *Kombinationsmethode* of Ebbinghaus is a form of the uncompleted word test; Ritter has proposed a form of our own familiar *A* test. There is little limit, other than the purely mechanical, to the experiments that can be applied in this way. But as has more than once been pointed out, there are great difficulties in the way of using the mere optimum performance in a given test as a measure of the state of fatigue. This optimum performance, especially in school work, is affected by entirely too many other conditions than those it is here used to measure. Even such elementary things as interest, distraction, and rivalry can have a considerable influence upon the gross performance, and are quite likely to vary independently of the fatigue process itself. The reader may remember the interesting observations of Schuyten (22), who experimented with auditory memory, testing the children morning and afternoon of the same day, and afternoon of the first day and

[1] Upon some of these points the writer has already touched, from a slightly different angle, in an article in the *American Journal of Insanity*, LXIV, pp. 502 ff.

morning of the next. Whether made in the morning or afternoon, the children did better in the first test than in the second. The only inference that is in any way justified by such an observation is that the factor of novelty was more potent than that of practice; but in motor accuracy the reverse condition might well have obtained.

Such experiments as these belong to the following type; an initial test a, is followed by a period of supposedly fatiguing work w, after which is made a second similar test a^1. The entire result is given in the difference between these two measures, a and a^1, and is absolutely dependent on their validity. The susceptibility to whatever condition is brought about by the work is given in the relation of the two quantities, but a^1 is not necessarily less than a; indeed, there are numerous instances from the literature in which the second test shows a marked gain over the first. And we must remember that the same relation between a and a^1 may be brought about by very different fatigue conditions. The measures a and a^1 have often been so unprecise, and the work done between them so ill-controlled as to give really no reliable criterion of susceptibility in the individual, and only a very slight one for the group.

But perhaps the greatest objection to be brought against this type of experiment is that it gives so little opportunity to distinguish between the individual who is already so fatigued as to suffer little fatigue loss during the experiment, and the individual who, though unfatigued, suffers as little through a relative immunity to fatigue. The only clue that it gives at all is in the gross scores, and, as has been said, the individual measures are so coarse as to make this criterion quite unreliable. The situation is complicated by the fact that the former class is likely to be made up largely of individuals highly susceptible to fatigue. But when we measure a fatigue in terms of itself, *i. e.*, study the actual fatigu phenomena of a certain function, there is reason to believe that we can differentiate these groups through factors largely independent of the gross scores.

And in this, I think theoretically justifiable, shifting of the viewpoint from the measurement of discrete states of fatigue to continuous determinations of susceptibility, the problem is otherwise considerably simplified. We largely eliminate the errors arising from the differences in attitude toward separate tests, because the measure consists of a single test. Of course, if the single test is a prolonged one, such errors are more likely to introduce themselves, but from the very fact of its being more extensive, they are less likely to lead to a false interpretation of the results. We are no longer attempting to measure the fatigue due to the continuous exercise of one function by its effect on discrete performances in another, a procedure

whose logic often suggests measuring the width of the mouth of the Amazon River by taking soundings in Chesapeake Bay; but we are using continuously a single function of determined efficiency of performance throughout. There would seem to be little room for comparison between the validity of the two procedures, always bearing in mind the question, is there such a thing as general susceptibility, and if so, to what extent may individual measures be expected to reflect it? In making such selections we are again in danger, to reverse Dr. Edes's expression, of inferring a rise of the Mississippi at the mouth from the occurrence of a thunderstorm somewhere in Minnesota. We are not yet, and perhaps never can be, fundamentally sure of how far it is justifiable to judge of a general fatigability through a determination restricted to a few narrow functions.[1] The most we can do is to select from the measures available for the study of fluctuations in continued work, such as shall best obviate the sources of error peculiar to this class of tests, as well as best meeting the more general requirements of psychological experiment.

There seems to be but little present reason for giving *a priori* preference to any special type of fatigue measures. Of course, if we were attempting to measure the fatigue of some special function we should naturally cast our test as nearly as possible into terms of that function; but using the term in a more general, or, if the word may be permitted, a more abstract sense, there is slight, if any, theoretical reason for preferring a motor test over a sensory, an intellectual over a motor. We must consider them upon their merits, as psychological tests. There is probably no test that entirely obviates any of the sources of error mentioned; but they are certainly subject to them in widely varying degrees, and differ widely in the precision with which they reflect ability in their special fields.

First of all, such a measure should make as few demands as possible on the conscious co-operativeness of the subject, because this introduces a large additional variable, absolutely uncontrollable, and of very ill-understood significance. The degree of co-operation accorded should be as constant as possible, and this end is probably best secured by making the degree demanded as small as possible. Kraepelin's addition test, which has figured so largely in the fatigue literature of the higher mental processes, has two considerable defects, and this is one of them. Each successive addition in Kraepelin's test requires conscious readjustments of no little complexity, and their in-

[1] We must also consider to what extent one's fatigability at high, experimental pressures of work may be correlated to that at ordinary, moderate pressures of work.

creasing irksomeness has been held in no small degree responsible for such fatigue phenomena as the workers with this test have observed. No doubt these readjustments tend to become automatic after a certain degree of practice is reached; but in ordinary fatigue experimentation we deal with individuals at practically the beginning of practice in such a test as this. In the earlier stages of practice consciousness cannot wander a hair's breadth from the work in hand without seriously affecting its amount, and these wanderings are equally difficult to prevent or control. In this respect it yields to the ergograph, though this instrument makes the frank assumption that "maximum" has the same meaning for consciousness at each pull. Moreover, the curves are considerably complicated with fatigue sensations; the weight instruments probably more so than the spring.

The feelings of annoyance arising from a long continued test make it desirable that the experiment should be one giving the requisite data in as short a time as possible. Here the Mosso instrument stands out best, and in the spring ergographs the characteristic phenomena also appear far sooner than in the tests of the higher mental processes, if indeed, as ordinarily observed, these tests show any objective fatigue phenomena at all. In this connection it is very interesting to note the method suggested by Squire (23), which consisted of the indefinite repetition of a rather complex motor act, recorded upon a kymograph. While the test was thus motor in character, the measure of fatigue was concerned with the higher mental processes, being given in an increase in the lapses and irregularities in the performance of the act. Considered from the utilitarian standpoint, the experiment requires a considerable time to make, and demands a degree of co-operation that would probably render it unavailable save among subjects of special training. A quantitative statement of the findings of such an experiment would be exceedingly difficult, in fact, none is attempted; and little practical value can be attached to a test that does not readily lend itself to this treatment. The study is most worthy of note as an attempt to analyze out experimentally a certain source of error, as that of muscular fatigue with the ergograph, and so to limit objectively the interpretation to be put upon the fatigue phenomena observed.

Thirdly, such a measure should be precise. This is the other weak point of the addition test. As at present given, we can measure the total amount of work done in a fixed interval, say 15, 30, or 60 seconds, or we can measure the time required to perform a certain number of additions, say 50. What goes on within these periods is absolutely hidden from us. We have no objective means of knowing whether a period of decreased

efficiency is the result of a gradual slowing down of the associative process, or through one or more extensive wanderings of the attention. Such elementary considerations cannot be arrived at unless the individual processes are recorded, and in the addition test this is impossible to do with any accuracy. Moreover, there remain the errors. As there is no way to deal with them, they are usually disregarded; but they are far from being shown to be negligible for the significance of the test.

Another factor must be mentioned, which is of vital importance where there is any question about the co-operation, as in clinical work. Unless the answers in the addition test are either spoken or written, there is absolutely no objective evidence as to whether the work has been done or not, save in so far as one might infer it from the figures of the results obtained. From these objections it will be seen that the ergograph is relatively free. The objective record of the work is there, and is not complicated with two such incommensurable factors as amount and error in the addition test. In precision, also, the ergograph record leaves little to be desired, though it must be remembered that this precision is not necessarily synonymous with accuracy, at least in the majority of the instruments. Nevertheless, so far as concerns the technical points of speed, objectivity and precision, there can be small doubt of a consistent superiority of the motor measures over the intellectual. Only in the simplicity of their apparatus have these latter an advantage, and this advantage is considerably discounted by the fact that it involves no corresponding simplicity of procedure for the subject.

It is worthy of note that so much attention has been given to the force of movement, to the relative exclusion of its other functions. It is difficult to imagine any *a priori* reason why the force of movement should have any special superiority as a psychological measure. Our primary aim is a measure of a neural process, and every one knows the extent of the discussion as to whether the ergograph is really a measure of nervous facts at all. On the contrary, we find in the maximum rate at which voluntary motor innervations succeed each other, a fatigue phenomenon so entirely foreign to what is observed unless nervous elements of some degree of organization are concerned, that there seems to be little, if any, escape from the conclusion that this phenomenon is at least mainly of nervous origin. Further, it is probable that on the purely technical side its experimental efficiency is at least equal to that of the ergograph. It is comparatively certain that it demands less co-operative effort; records of a considerable degree of trustworthiness have been obtained from depressed and demented subjects who would have been quite unamenable to ergographic

experiment. Sensations of fatigue at any stage of practice seem to influence the results to a minimum that is not approached in any other fatigue experiment with which the writer is familiar. Neither gross efficiency nor susceptibility to fatigue seem correlated to any significant degree with the subjective estimate of speed or with the sensations of fatigue that accompany the work. In the time required by the actual experiment, the test also makes a favorable showing; quite sufficiently characteristic fatigue phenomena normally appear during a period of 30 seconds, which interval has been selected as the standard in the writer's experiments. In using the simple tap, its ultimate precision is probably inferior to that of the ergograph, though it is quite equal to it so far as the actually evaluated factor, *i. e.*, the number of taps is concerned. Moreover, this objection could be entirely overcome by using a myographic instrument, as did Binet and Vaschide. There remains the tediousness of evaluation, in the counting of the individual taps; but it is probably better that the evaluation should be tedious than that it should be relatively unintelligible, as in the ergograph, or relatively meaningless, as in the addition test.

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LAURENS PERSEUS HICKOK

By JOHN BASCOM

Of all the men whom I have known Laurens P. Hickok was best fitted to command high and uniform respect. One might be drawn to him or remain relatively indifferent to him, but once within the circle of his influence one could not do otherwise than cherish for him warm esteem. He was not a man whose affections and passions nestled close together, or whose imagination, like a strong wind, bore him and others with him in new and unexpected directions; but his feelings and his thoughts were so vigorous, so perfectly interfused, so transparent in their scope, that they retained the attention they had once arrested. Sentiment was so dissolved in a fitting intellectual medium that one hardly thought of it, or felt it, separately.

This mastery was the more remarkable as he had occasion to present a system of theology which has made so much or authority, and which, although appealing to reason, has surrounded itself with so many narrow, personal and conventional motives. His own mind was satisfied with the inherent rationality of his beliefs, and on this ground he habitually presented and enforced them. He felt them to be perfectly open to inquiry, and that the reasons on which they rested were an essential part of them. He invited investigation as the fitting medium of conviction. He stood frankly and fearlessly on the basis of truth, feeling that only thus could that assurance be reached which comes to every mind in the successful use of its own powers; an assurance quite beyond any gloss of rhetoric or dexterity of logic. Personal motives weighed so little with him that he was hardly aware of their existence in others. That self-consciousness, that consciousness of surrounding circumstances and claims, which even good men with difficulty escape, left him unembarrassed; alone with the truth, his own thoughts and the thoughts of others concerning it. What he said was his personal conviction. No matter from what source it originally came, he was only interested in opening up to the minds of others this one royal and independent road. The air is air for us all, and native to every man's lungs.

I first met him in the fall of 1851, holding the chair of systematic theology in Auburn Theological Seminary, which at

that time commanded a good attendance. It was the most important position he ever held, and the one best fitted to his powers at the period of their full vigor. There is no weightier demand ever made on the mind and heart of a teacher than that made by young men, finishing a course of study by a consideration of the spiritual problems which determine the significance of life; problems which are to define our own action toward life and help us in our guidance of the actions of others. The student may have made many acquisitions, have entertained and passed through many inquiries, but these last questions, for the sake of which all other questions have been put, remain to receive a fitting answer, casting light before and behind till the day dawns upon the waiting spirit. It was in this temper of expectancy that my class was present, and however far off and ineffectual the conclusions reached may have been, they were sought after and waited for as cold water by a thirsty man.

Laurens Perseus Hickok was born in Bethel, Fairfield County, Connecticut, December 29, 1798. He was brought up on a farm and came into possession of a sturdy physical constitution which stood by him during a long life. He was graduated at Union College, studied theology and entered on his ministerial work in 1822. He first preached at Kent, a small township in Litchfield County, Conn. He was called thence to Litchfield to occupy the pulpit vacated by Doctor Lyman Beecher. In 1836 he was invited to take the professorship of theology at Western Reserve. Here he remained eight years, and then accepted the same professorship at Auburn Theological Seminary. This he also held for eight years. As a preacher he was simple, thoughtful, effective. Instruction was imparted and reflection called out by his discourses. In 1852 he became vice-president of Union College, with the understanding that he was shortly to become president. This expectation was not fully met. He remained at Union till 1868, and was not in complete charge till the last portion of the time. His earlier work was restricted by the presence of President Nott, an able but a very different style of man. Moreover the administrative duties of a college were not those which gave freest play to his powers, nor was the instruction of undergraduates equivalent to that of a seminary class. His presidency at Union, his alma mater, seemed to be the crowning feature of his life's labor, but was hardly so in fact. While it was successful it was less genial and rewardful than previous work. He resigned in 1868, and removed to Amherst, an educational centre and then the home of his nephews, Julius H. Seelye and L. Clark Seelye, who were professors at Amherst College. Here he spent most peacefully and happily his last

years, cherished in the affections of his many pupils. He died May 7, 1888.

Dr. Hickok's philosophical labors, for the sake of which this sketch is written, were very definite and closely interlocked. His first volume, Rational Psychology or the Subjective Idea and Objective Law of All Intelligence, was published by Derby, Miller & Company in 1849, Auburn. It contained the prominent conceptions of his later volumes. It gave rise incidentally to two manuals, Empirical Psychology and a System of Moral Science, used by himself at Union, and also by President Seelye at Amherst. In the line of an extended application of these principles of knowledge came, in 1858, Rational Cosmology, in 1872 Creator and Creation. In the same year appeared Humanity Immortal; Man Tried, Fallen and Redeemed, a psychological justification of his theological system. His work was closed with The Logic of Reason; True Logic must Strike Root in Reason, 1875. His later volumes were published by Lee & Shepard, Boston. Doctor Hickok did not depart materially from current orthodoxy, but he gave it interpretation and support by his rational conceptions of the nature of knowledge. He put his own mind at rest on what he felt to be the final statement of human powers. While, therefore, he might approach other systems of doctrine in a formal presentation, his own creed was offered as a universal solvent of ethical, spiritual relations. He urged it as rational, through and through.

His Rational Psychology took form in the mind of Doctor Hickok when the Transcendental Philosophy was in the process of development in America, a philosophy which seemed to him to combine fresh insight with wayward and vagrant thought, and to call for a new definition of the nature of knowledge. This he undertook to give in his Rational Psychology by a sufficient statement of the necessary conditions of knowledge, of perception, understanding and reason, all standing fast with and by means of each other. The foundations of truth, its ultimate validity, were to be disclosed and vindicated against all confusion and scepticism. He felt that the paths and methods of spiritual thought could be laid down with something of the same exactness and certainty with which the mathematician follows the clues of space relations. It was a fresh survey of the highways of knowledge on which he entered.

There was also in the air the dogmatic assertion of commonsense which went with the Scottish Philosophy; an assertion of certainty rising above analysis and resting on the accumulated convictions of men. It affirmed that knowledge as one whole stands or falls together, that we cannot break in upon it and trample it under foot without scattering it altogether.

This inherent conviction, which the school of Reid dogmatically affirmed, Doctor Hickok strove to bring out into the light of reason and to disclose the self sufficiency of knowledge as one whole. Though its several parts may suffer constant readjustment, these changes, no more than the ever renewed elements in the physical body, do not prevent it from being in the full possession of life.

An empirical psychology is enclosed in a rational psychology as the kernel is contained in the shell. What the latter affirms is that these mental powers, empirically discovered, are the germ of a complete life, self sustained in all its parts. The independent and creative quality of intellectual activity is pointed out, and we are led to see that we are in possession of true knowledge according to the obvious purport of our daily experience. The mind is justified to itself.

There are three forms of mental activity: that of the senses, furnishing the material of perception; that of the reason, giving the forms of knowledge; and that of the understanding, uniting these two in judgments. Each of these three is discussed in reference to its constituents. The elements of knowledge due to the external world, the elements due to the mind itself, and the completed product due to their union in thought, are brought out in their equal and adequate authority.

The chief ideas, or form elements by which crude sensations are transformed into clear perceptions are space and time. These are in no way contained in the sensations themselves but are brought by the mind to that mental construction by which these sensations pass into completed terms of knowledge. The sensation is sterile without the form, and the form is empty without the material given by the sense. Both are dependent and independent; both are essential parts of our knowing. All the efforts of the empiricist to obtain either of these form elements, space or time, in sensation itself are merely a cunning rehearsal of the conditions by which they are called out in the mind. All organic response, or instinctive response, of the physical man to the impressions of sense fall short of knowledge till the rational construction under these rational forms has taken place. Faithful analysis gives them both, and is compelled to refer them to different sources, the sense and the reason respectively. Thus perceptions, the earliest product of the mind, which are to lie at the foundation of knowledge, are found to contain constituents from the external and the internal world, blended by reciprocal and inseparable action in one transparent product with the same certainty with which oxygen and hydrogen combine in water.

These first and relatively simple materials of an intellectual world come at once under the constant and more extended

action of the judgment, till they form parts of the general scheme of nature; constituents in our experience more or less extended according to our forms of inquiry and of activity. This addition of units, this weaving them together into the intellectual fabric of life, proceeds under the form elements of substance and cause. The reason adds to each group of phenomena the sense of reality, the feeling of an underlying something, more frequently called substance, which remains as the constant source of these impressions. The impressions and the reality are the equivalents of each other and make up a firm and uniform experience.

Associated with this notion of substance is that of causation. The forces involved are not passive but active forces, which govern phenomena in an orderly procedure, in a form of activity defined by existing circumstances. These two form elements of substance and cause enable us at once to build up a knowledge of experience more or less extended according to the attention we give it. This knowledge, which we are especially wont to call knowledge, contains, like perception, two different and distinct elements, one finding entrance through sensation and one through reason. Without the constructive form, knowledge is blasted in the bud; without the fixed terms of phenomena, it becomes visionary and fanciful, clouds which disappear while we look at them. Here, again, we have the two sources and constituents of truth, neither of any validity without the other.

So far the inquiry is kept within the ordinary forms of knowledge, and we find that the things we see and feel give rise to perceptions and judgments which contain two elements, parts contributed by mind and matter in reciprocal action. Our construction of events into an intelligible experience thus becomes a joint product of disparate agents. What men have everywhere and from the beginning regarded as knowledge, knowledge self consistent, reliable and capable of increase, is found, under clear and fair interpretation, to contain two forms of activity, and to reach completeness by means of them both.

We now come to transcendental inquiries, which the human mind is ever making, led to them by other form elements which remain to be filled out, and which carry the thoughts forward to their most comprehensive expression. The mental forms under which this wider survey goes forward are equally plain. The first of them is personality, that assemblage of powers which, under a universal rendering, give us the sense of manly endowment, spiritual presence. Dr. Hickok conceives this soul-power as involving unity, spontaneity, autonomy and liberty. They are not so much separate parts of one life as separate ways in which one life may be regarded. A pure

intellectual process, that in which the mind of man is most detached from physical relations, offers itself as something wholly distinct from a series of causes. Take the conception of a circle and a discussion of its qualities. Any affirmation we make concerning it, as that the areas of circles are to each other as the squares of their diameters, is established in a sequence of thought which has no resemblance to a series of causes, as when a rock is split by repeated blows. It is a process of its own order, a thought process, which justifies itself to the mind, and owes its conviction wholly to that fact. Such a movement of mind is spontaneous, autonomous and free. We often speak of liberty as if it found sole expression in an act of will. The truth would seem rather to be that liberty belongs to mind as mind, pursuing its inquiries according to its own nature, and making them less complete or more complete according to its own purpose. A process of thought cannot go forward without this liberty and not lose its own character, its own connections, its own integrity.

One of the most obscure and controverted directions in which the reason of man brings form to human judgment is the law of right, the law of righteousness. Such a law lurks in men's mental processes, in one direction or another, with one or another degree of completeness and of authority,—the more thoughtful the man the more authoritative the law. This law of right, which enters so often to constrain the actions of men, is closely associated with happiness, with conventional sentiment, and with the forms and enforcements of social and civil law. Yet it transcends them all, struggles to reconcile them all, and gives to them a distinctive pungency which they cannot otherwise obtain. The law of right is constantly changing its form and direction in men's minds, absorbing into itself many considerations of pleasure, of the growth of individual and general prosperity, and yet remains supreme over its constituent and associated ideas, inexpugnable, ever reasserting itself above all denial and beyond all explanation. The sense of right in many persons in reference to many forms of action, asserts an authority which they cannot escape, and which adds to disobedience a sense of shame and humiliation they cannot soften. The magnetic needle may easily vacillate and be much deflected from the pole, yet it still remains a constant guide in our voyages. The lines of the spectrum, aside from any explanation, are fixed characteristics of the elements which involve them. This sense of law, more supreme as men advance in intelligence, is inseparable from the rational mind in its outlook over the spiritual world. We may offer as a guiding principle, the greatest good of the greatest number, and yet it is the sense of right which interprets the precept,

not the precept which shapes the sense of right. The sense of right still remains to us to tell us what the greatest good is, and to enforce upon us as a law of action this good in reference to ourselves and in reference to others. A sense of law, of supreme law, still to be expounded in many directions and to be enlarged as it is expounded, remains with us; and by virtue of it the world becomes ethical, a field undergoing, slowly it may be but certainly, a spiritual construction. A universal expounding law is present which works its way experimentally and rationally into the government of the world, and makes of it the Kingdom of Heaven. Thus we find the world to be a spiritual kingdom whether we have or have not recognized its King. A notion which transcends all final statement is with us to give form to our lives.

If we add to freedom, exercised under law, both of which we refer to the spontaneous activity of mind, the notion of the infinite and the absolute, we are able to give to the universe, to the largest procedure of the world, an exposition which, like a discussion of the properties of a sphere, may still leave out many things we would be glad to know, yet holds for the mind the germs of truth which quicken and gratify to the utmost our spiritual life. Under these conditions the sense of intelligence, and ultimately of a Supreme Intelligence, comes to us as the true power of this system of things of which we are a part. With it arises a belief in immortality as alone giving room enough and incentive enough to carry forward these impulses, which we are beginning to understand, into a kingdom proportioned to their own magnitude.

We both see and feel that there is an evolution, and that this evolution is one which will justify all the ways of God toward man. These conceptions make the world supremely rational, though the lines of order may so far have been traced by us inadequately and obscurely.

This is but a summary presentation of the Rational Psychology, a volume of seven hundred and seventeen octavo pages, containing besides its primary theme many subsidiary illustrations and criticisms. The mind of the author was so thoroughly occupied with the constructive force of the human mind, that he proceeded at once to bring its regulative thought to the interpretation of the physical world about us. The first volume in this direction was Rational Cosmology, published in 1858, presenting a speculative physics and biology. In 1872 it was followed by Creator and Creation. These are extraordinary works in the sustained attention they imply, and in the extent of the phenomena which they embrace. They can receive from us only a brief presentation.

The preface of the Rational Cosmology contains a statement

which foreshadows the purpose of the volume. "It will thus ever be true of the finite human reason, that with the mere facts of nature he can never rise to any science of nature, and with the partial apprehension of the principle he can never follow it out in all its necessary determinations, and, hence, his only sure progress must be, first, an apprehension of the principle, more or less inadequately, and then a following out of the principle in its necessary laws by a reference to the actual facts that have already been determined by it. . . The facts are nothing for philosophy except as seen to be determined in their principle, but are much for philosophy when used by the insight for the development of the determinations of principle." The first chapter expands the idea of a Creator and the second and third chapters, constituting the body of the work, give a brief statement of the conception of matter as a product of mind, and then proceed with an extended tracing of the forms and laws of matter as known to us in a large variety of departments. The volume is a tentative application of a rational idea of matter to the various facts which are united under it in the general system of things. Thus the world becomes intelligible throughout by an apprehension of its controlling principles.

The second volume, whose contents evidently grew into distinctness as the author's mind, in the progress of years, came to dwell on these primary conceptions, elaborates more fully the fundamental nature of matter, and the construction which would arise under the antagonistic, diremptive and revolving forces which lie at its centre, and issue in a creation; a preparation for the various forms of life, and a rational product springing from the reason of God and addressed to the reason of man. The second volume covers the same primary ideas as the first volume, and employs them with the growing distinctness and confidence which arise from long meditation on them. The two books give a very clear example of what frequently occurs in philosophy. Increased familiarity comes to be the equivalent of increased proof. The mind conceiving its ideas ever more clearly is proportionately impressed with their explanatory power, repeats them under their later forms and justifies them afresh with each enlargement of light.

The second preface contains this statement in line with the one already given. "We must recognize a higher spiritual faculty than sense-experience, as an organ of spiritual philosophy which shall abundantly comprehend and confirm our theology; and therein may all scepticism be fairly met and answered. The phenomena of nature must be seen to be ordered by essential forces back of the appearances; and also faith in theism must rest on truth known to be beyond nature

and determining the order of nature, though known by the insight of reason in nature."

In the same year, 1872, in which Creator and Creation was published, appeared Humanity Immortal; or Man Tried, Fallen and Redeemed. This work came as a natural completion of Rational Psychology and Rational Cosmology. It gave the final defense and support of the system of theology which the author had for so many years presented. His conception of the divine character and of human liberty led him to regard human destiny, man's relations to God, in their most inspiring form. The comprehensible and the hopeful received far more emphasis than the obscure and portentous, and thus it became a most assured and grateful issue, that we, too, should reach the presence of Pure Reason.

The closing volume, Logic of Reason, was published in 1875, and was a return to the key of the whole structure. Dr. Hickok was so profoundly occupied all his life with his ruling idea that no additional light was without interest to him, and no repetition disturbed him. Enlarged apprehension came to him as superior light on the spiritual landscape, a pushing forward one step further toward the coming day. Dr. Hickok had also such a constant sense of the partial and inadequate presentations of truth in current forms of philosophy that some critical and constructive and corrective process was ever arising in his mind, and pushing forward in immediate, effective use.

Thus the Logic of Reason opens with a full discussion of transcendental and of empirical logic, and then passes on "to discuss those forms of force and life which are the substance and essence of the universe." Reason is put to the test of reading in the facts of experience the principles conditional for it, the things which have preceded it, and to determine the order of inherent, adherent and coherent connections in all phenomenal observation. Whatever else we may think about the philosophy of Dr. Hickok, he did not propose to himself any secondary or insufficient purpose.

We are not to conceive of the philosophy as thrown off at a single heat. It was rather a growing light to which the eye became more and more accustomed, by means of it gaining a clearer revelation of the visible and invisible world. Hence the constancy with which he returns to it, and his sense of something more adequate in each fresh presentation. His mind worked like the mind of Spinoza, intoxicated with a conjoint vision of nature and God. During a long, spiritual, thoughtful and peaceful life, the dawn of reason passed into the dawn of philosophy, and the dawn of philosophy into full day in which one seems to see even as he is seen. It is no

more necessary that the thought by which we apprehend the mind of God should be altogether perfect than it is that the light which discloses the world should be everywhere present and complete.

I am not willing to close even this brief sketch of the system enforced by Dr. Hickok without some estimate of its validity. The Rational Psychology, passing by secondary and verbal criticism, seems to me to go straight to the root of our intellectual life. We cannot, by any adequate analysis of our sensuous experience, reach the forms of thought which belong to a rational handling of the world. Moreover, this joint product which arises between mind and matter is in harmony with what we meet everywhere. No finite cause by itself alone creates and gives character to an effect. In every effect, every combination, we have at least two agents which concur in the result. Nothing lies wholly passive to receive the action of other things, wholly active. We have also in this philosophy of reason a vindication of the fundamental constitution of the mind and an acceptance of universal knowledge. Man, in the repeated and spontaneous use of his powers, has reached results which combine these two elements, physical fact and intellectual interpretation, in a manner accordant to this conception. Nothing which disturbs the foundations of general knowledge can be conceded, for this is to render knowledge self destructive.

A most important criticism to be made on this portion of Dr. Hickok's work is that he speaks of consciousness as an inner sense. It may much better be regarded as itself a form element, essential to a large class of phenomena; those we know as intellectual phenomena. What space is to physical facts that is consciousness to intellectual facts.

When we come to the use made of this psychology in cosmic interpretation, conviction drops off. The view presented casts light only here and there by accident, as it were, and leaves the great mystery of method untouched. My first contact with Dr. Hickok was, as I have said, in 1851, in the seminary at Auburn. I had already begun to find difficulties in the religious dogmas in which I had been brought up. The atonement seemed a human device consistent neither with ethical law nor God's revelation in daily affairs. I was glad to give weight to Dr. Hickok's opinions as relieving an uncomfortable pressure. He did not attempt to overcome my doubts by any severe rendering of the idea of justice, but urged that the atonement was to the human mind the most obvious and adequate presentation of the divine mind toward sin and toward men. Rejection of sin on the one side and grace on the other were clearly and adequately brought out in this doctrine. This

reasoning for a time satisfied my mind, but later the old difficulty revived. This method was not in harmony with the government of the world, the government expressed in the parable of the prodigal son; not in harmony with the fact that repentance always carries forgiveness with it. Thus once more dogma became a scheme put upon the world and not found in it.

I had from the beginning much the same feeling in reference to his conception of the forces, antagonistic and diremptive, by which he proposed to explain the universe. They brought their own difficulties and gave no revelation as to the nature of things about us. When we are dealing with the most familiar phenomena, we get quite beyond our depth, if we undertake to put another series of facts back of them. Take, for example, a ball flying through the air under a blow from a club. Does the ball differ from what it was before it was struck, and how will it again differ when it is brought to rest? The facts are uniform and plain, but how came they to be exactly what they are? It helps us very little to imagine the ball in motion to be charged with something we call force, and to lose it again when brought to rest. The acquisition, the intermediate state and the loss still remain mysterious. To inquire exactly what the phenomena are, that is science; to see the purpose subserved by them, that is philosophy, but any imaginary intervening mechanism is not even knowledge. When we come to the how of things, it is to get over the insuperable difficulty which arises in the mind that we assert the presence of Infinite Spirit. We can give to intermediate notions of matter and force no such form as to remove the sense of ignorance we experience the moment we reach them. It is equally a false form of knowledge to deny the reality which we cover by matter and force and to attempt to explain it. True philosophy stops when it is through, and offers no explanation of noumena beyond the noumena themselves. The noumena are not in the explanatory series, but superinduced upon it. We accept them as inseparable suggestions from phenomena, known in the degree in which we know the phenomena, and playing their entire part in connection with them. What we know as force has many phases, and each phase finds expression exclusively in one set of phenomena. The more absolute this identification of each force is made with its own phenomena the more adequate our knowledge becomes. We are not, like the Indian, to explain the steam engine by a horse enclosed within it; the engine explains itself. The simplest exposition of gravity is the bare fact that ponderable bodies attract each other. This statement is the substance of what we know, and all the knowledge of which we can make any use.

Dr. Hickok is by no means alone in putting notions back of notions, vague realities behind obvious realities. Yet no philosopher advances one step in knowledge by this method. Take the doctrine of inheritance. What we need is the facts in fuller measure, to accept them in their own form and to keep away from them any unverifiable conceptions—conceptions which are sure to affect our estimate of the facts themselves. Gemmules and physiological units may seem to help the doctrine of inheritance, but before we are aware of it they will divert attention from what is, to what ought to be under their intermediate agency. We are overrun just now in psychology with fanciful forces which we can in no way locate or expound. Visions and suggestions, shaped in an unconscious region, come crowding up by a subliminal passage into the mind where they serve to confuse in wonderful ways the actual phenomena of intelligence. The more we shape for ourselves such appliances the less we shall know of what are the actual, verifiable relations of thought and physical facts; we shall slip away from the world of realities into one of shadows and moonshine. The fact and adherence to the fact are the first lesson of sound inquiry. If what we term "new psychology" were called neural dynamics we should know much better what we are about.

While pure mathematics is an obstacle which the empiricist has never been able to push out of his way, it has also at times been a snare to the intuitionalist. Its conclusions are so absolute, so independent of experience, that it seems to move in a region quite above facts. Its units are perfect units, its definitions without ambiguity, and, wherever numerical form alone is involved its conclusions are unimpeachable. When, however, in mixed mathematics the discussion pertains to real things, the absolute quality disappears. We manage with much labor to secure proximate units in weights and measures, but when we come to estimates of mind, as in values, we are quite at sea. The purchasing power of a certain number of dollars is constantly in fluctuation, has no measure. It turns on the form of currency, the state of credit, the market in which it is tested, the articles purchased, the state of mind of buyer and seller. Values, like tides, are coming in or going out, and are liable to play us unexpected tricks.

When we reach social or artistic or intellectual or virtuous quality our numerical estimates are so much at random that we rarely offer them. Statistics, their most common form, themselves need interpretation before we can by means of them either affirm or deny anything.

We are disposed to push mechanical notions and measurements quite beyond their application, simply because they do

serve at times to render our knowledge so complete. Even the universal belief that matter is indestructible is an induction resting on relatively few facts. The equivalence of forces is by no means the broad principle which we sometimes think it to be. It is not an assertion that forces in distinct amounts displace and replace each other, but that a few forces, under mechanical relations, may be made the means in expenditure of calling out other forces of a like character. Fuel consumed in a steam engine is not replaced as regards force by an electric accumulation, or by mechanical energy. In its own dispersion, it gives rise to a distinct accumulation of force which may in part take its place, awaiting in turn some other form of dispersion. The locomotive runs one hundred miles. It might have run more or less, or stood still on the track. The heat, ready to produce motion, is lost, and does not reappear in any other form. There is a conditional dependence of one form of force in its generation on another form, but each remains subject to its own circumstances. In the solar system forces are constantly suffering dispersion in large amounts.

The forms of undue extension given to conceptions by the empiricist and by the intuitionalist are often much alike. In the discussion of the origin of species, there are two considerations of very unequal importance. The first is how come there to be suitable varieties, in the character and amount of the changes involved in them? The second is the tendency of these varieties to survive. The survival of the fittest approaches a truism. No one is disposed to deny it, and it is kept in the foreground. How the fittest comes to be is a much more difficult inquiry, and is inadequately treated. The intuitionalist can readily, if any given effect is to be secured, insist that means, akin to those actually employed, become necessary. This assertion is not expounding why, in spaces and periods unoccupied, these results, or indeed any results, must arise. A movement once started, a certain power of prediction goes with it, but whence the movement itself arises is not a forecast of our rational powers. So it is in any affirmation we can make concerning any actual event, the necessity lies in the relation of one part of it to other parts of it, and not to it as one whole. A strong argument for a Divine Agent may be framed from the profound and innumerable interdependencies of the physical and the intellectual worlds, from the fact that they together unite in one universe; but any necessity which goes to show that these results are inevitable, locked up in antecedent connections, reduces not enlarges our sense of the need of a Divine Presence.

Our true attitude seems to be a close and extended inquiry into things as they are, a sense of the mystery of that power

which, point by point, makes them what they are. These are the facts with which we have to deal. A universe according to Spinoza, or according to Spencer, or according to Hickok may seem for a moment more intelligible, but is in no way so instructive, so stimulating, as a universe in which the mind moves freely, a universe constantly disclosing new phases of power.

Many theories to which we are disposed to attach importance simply hide that eternal development of thought from thought, method from method, which confronts and feeds the mind, and puts it on terms of intercourse both with the known and the unknown. We certainly may know all we can know, see all we can see, but the thin mist of speculation we spread over the landscape often conceals far more than it reveals. It has been affirmed by one entitled to an opinion that the philosophy of Dr. Hickok is the most comprehensive and original of any, which up to his time had appeared in America. This statement seems to be just, if we consider the circumstances under which it arose, its scope, and the soundness of thought and method.

It was brought forward as a defence, on the one hand, against the stolid assertion of Scotch philosophy and, on the other, against the volatile speculation of Transcendentalism. The two manuals, one on psychology and one on ethics, marked an era in instruction in Amherst College and became influential elsewhere.

This system also covers the entire field of philosophy. Dr. Hickok, during a long life, had in mind the complete philosophical problem, psychologic and cosmic. He was profoundly interested in any philosophy which strove to cover and support human knowledge in its several forms. This interest is seen in his frequent criticisms, and in his extended treatment in his Logic of Reason of the various comprehensive forms of thought hitherto offered. His feeling was that philosophy stood for a well rounded system applicable to all forms of inquiry. It was this conviction that led him to take up a second time, after so long an interval, the cosmic problem.

The justness of his general method, notwithstanding the undue extension given it in physical discussions, must be freely conceded. His seven volumes, all devoted to one self-assigned task, started in psychology, a study into the scope of human powers, and at the close returned to it in the Logic of Reason. Though the rational element was given the foreground in every inquiry, yet the method of approach was primarily empirical. His rational and empirical psychology alike rest on a careful analysis of the facts of consciousness. Knowledge in its incipiency is shown at once to hold the empirical substance and the rational form of truth. His cosmology, not-

withstanding its speculative scope, deals directly with the forces of the world, attractive and diremptive, and strives to make out of these the substratum of all physical things. Dr. Hickok thus achieved a comprehensive system of philosophy, pervaded in all its parts by a temper both rational and empirical; a system venturing further and with more sobriety into the field of knowledge than any which had gone before it in America.

THE EFFECT OF PRACTICE IN THE CASE OF A PURELY INTELLECTUAL FUNCTION

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The mental multiplication of one three place number by another affords a convenient means of studying several interesting psychological topics. For instance, the process affords perhaps the best brief test of attention of those so far used; the nature of the images in which one thinks is shown perhaps better by such a real mental problem than by questions concerning one's power of voluntary recall of images; the efficiency of the process is readily measurable so that it serves well as a test of fatigue or practice. It is especially advantageous for the study of practice because it requires no apparatus and offers a case of improvement in a function which a student of very slight psychological training can readily understand and measure. The experiment which is reported here might well be made as a part of the class work of a course in psychology.

I shall not rehearse all the details of the management of the experiment or all its results, but shall confine this report to the facts necessary for the understanding and criticism of certain conclusions concerning the amount, rate, progressive change of rate and spread of improvement.

THE EXPERIMENT

After preliminary training with three or four examples in mental division of a 6 place by a 2 place number, and two examples in mental multiplication of 3 place by a 3 place number, 33 individuals multiplied mentally from 50 to 96 examples like those quoted below,[1] which are a random selection in random order of the examples made by putting any 3 place number containing no digit lower than 3 and repeating no digit, with any other such 3 place number.

Of the 33 individuals 1 did only 50, 1 only 60, 1 only 66, 1 only 75, and 1 only 85 examples. The remaining 28 did 96 each. In what follows only the 28 individuals will be considered, unless a special statement to the contrary is made. As a rule 5 or 6 examples were done per day. The time of day varied amongst

[1]

657	398	479	358	589	395	396	864	739	983
964	367	476	537	745	359	953	659	459	394

individuals and in some cases within the different practice periods of the same individual. It was impossible to prevent these variations in conditions without imposing great inconvenience on the subjects. Such variations increase somewhat the variable errors of all the determinations, but if one is careful to interpret differences in results with full awareness of these differences in conditions, no serious harm need result.

Each example was done as follows. A time at which to start was set and recorded. At this time, say A. M. 8 hrs. 40 min. 30 s., the example was taken up, looked at long enough to fix the two numbers in memory so well that they could be repeated from memory and further memorized without the paper. The example was then laid aside, no sensory aids were used, and when the full answer was obtained it was written down and the time recorded when the last figure of it had been written. If the subject was interrupted *ab extra* as by a knock at the door, the record was omitted, the same example being done a day or so later. The subjects were allowed to examine their results in comparison with the correct answers.

Reduction of the Scores to One Variable

For the purposes of this article the following scores were used: (1) The times taken in doing the 1st, 2nd, 3rd, 4th, 5th, 91st, 92nd, 93rd, 94th, 95th, combined times for 1-5, 91-95, 86-90, 1-10, 11-20, 21-30, 31-40, 41-50, 51-60, 61-70, 71-80, and 81-90. (2) The errors made in each of the above examples or groups of examples, an error being defined as any wrong figure in the answer, 6 errors being the worst possible record for an example in accuracy.

When it is desirable to have a single measure of efficiency, I transmute errors into time by adding 1/10 of the time taken per example in lieu of each error made. Thus a record of 200 seconds and 1 error for an example becomes 220;—a record of 2,500 seconds and 13 errors for ten examples becomes 2,500+($13\times\frac{250}{10}$), or 2,825. Any such scheme of allowance can be criticised and I do not pretend that this is the best one that could be found for the present case. It is not far wrong, however. The gross figures are given in Table I so that any one who chooses may apply any other scheme for equating time and errors. It will be found, I think, that with any rational scheme the general conclusions of the study will remain as they now are.

The Amount of Improvement

The facts from which the amount of improvement is estimated are the records of the first five examples done and the first five of the last six done, taken in connection with the time of day when it differed in the two cases. By observing

Table I.—Gross Scores

Individual	Sex	First Five		Last Five		First Ten		Second Ten		Third Ten		Fourth Ten		Fifth Ten		Sixth Ten		Seventh Ten		Eighth Ten		Ninth Ten	
		Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.	Sec.	En.
1	F	1022	16	764	1	2004	20	2565	20	2285	10	2259	27	2205	14	2081	16	1826	21	1528	21	2093	23
2	F	1243	22	680	18	2298	44	2500	38	2426	35	3131	35	2864	40	2705	48	1688	38	1703	37	1709	26
3	F	2040	2	880	3	4200	9	3120	15	3305	15	2070	7	1990	14	2220	7	2058	11	2412	18	2406	2
4	M	2130	7	600	1	3390	13	2580	6	1740	8	1275	6	1050	2	1020	7	1110	2	960	4	990	7
5	F	3000	12	1230	5	6300	24	4700	32	3550	23	3240	24	3375	15	2950	16	2730	15	3300	18	2685	12
6	M	1315	5	867	3	2422	15	2433	13	2470	19	2352	12	2313	18	2112	17	2053	19	1865	14	1862	10
7	F	2185	16	1803	5	5500	22	7630	26	6877	16	8483	14	6800	11	6248	9	6519	12	4440	13	4452	7
8	F	1980	14	990	11	4440	25	1950	40	2400	25	2160	13	2670	20	1785	17	1395	23	1855	16	2080	15
9	F	1370	15	550	2	3181	27	2493	27	2100	14	1615	18	1701	13	1400	15	1327	16	1010	14	1085	6
10	F	2400	12	1230	10	3570	17	3375	6	2778	8	2445	4	2445	27	2527	11	2305	14	2167	10	2400	21
11	F	3535	12	1915	5	6442	23	5331	19	5317	17	5160	21	4135	14	4231	20	4108	18	3548	17	3656	10
12	F	2834	23	1511	10	5616	40	6437	4	4924	18	4196	18	5250	12	4289	19	3320	9	3286	16	4074	15
13	M	2765	6	377	6	4695	13	3447	15	2461	11	2165	10	1988	7	1597	11	1256	8	1109	15	947	10
14	F	900	3	525	3	2200	5	2765	8	2945	3	1895	8	1935	4	2105	8	1660	8	1275	8	1170	4
15	F	5340	0	2369	0	9180	1	9090	10	6293	11	6690	13	5387	7	4702	5	4495	5	3932	7	4480	6
16	F	1870	12	450	1	3505	22	2855	18	2285	20	2065	9	2467	10	2045	13	1470	12	1700	16	1315	13
17	F	2665	19	637	10	4535	37	3532	21	3201	19	3260	17	2295	15	2270	11	1823	14	1675	22	1720	18
18	M	3000	16	960	9	6120	28	4800	13	4080	16	4560	10	3420	21	3120	14	2760	21	1740	25	2460	20
19	F	2235	4	1080	0	3915	10	3660	8	3660	9	3225	9	2385	6	2370	5	2400	3	2265	5	1905	4
20	F	2040	15	825	3	4020	26	4105	8	3060	18	3375	10	2550	13	2355	5	2970	8	2565	21	2055	12
21	M	2530	11	725	17	4325	26	3825	31	3323	29	2655	32	2845	25	2365	23	2340	18	1960	30	2220	27
22	M	2368	2	971	0	3792	2	3553	4	3079	4	3258	6	2912	0	2845	5	3085	2	2294	2	1939	0
23	F	1590	13	950	14	3450	16	3790	17	3245	16	2365	5	2025	12	2290	9	2520	17	2050	15	2260	9
24	F	1857	2	740	12	3657	10	2801	23	2843	9	2347	15	2601	9	2503	19	2334	10	2232	11	1603	18
25	F	3010	21	1500	7	5135	36	4700	26	4200	29	3780	19	2990	26	4084	19	3460	21	2630	25	3760	22
26	M	1565	6	780	5	2710	15	2225	17	2465	20	1955	11	2145	18	2205	12	1750	5	1645	3	1870	11
27	M	2720	5	820	4	4870	13	4000	18	3270	10	2390	11	2615	10	2400	13	2430	16	2160	11	2280	15
28	M	3525	9	1022	10	6340	15	4430	22	3631	16	3174	18	2960	13	2688	16	2418	13	2394	13	2139	15
29	M	......	....	......	....	3750	11	3580	21	3355	21	2348	14	2166	6	......	....	......	....	......	....	......	....
30	M	......	....	......	....	4145	13	3281	11	2379	9	2405	10	2008	19	1550	11	1660	13	1700	12	......	....
31	M	......	....	......	....	4672	7	4448	2	3693	6	2154	7	2019	2	1798	3	1428	4	......	....	......	....
32	M	......	....	......	....	13157	2	13115	12	......	....	......	....	......	....	......	....	......	....	......	....	......	....
33	M	......	....	......	....	8965	2	6610	5	......	....	......	....	......	....	......	....	......	....	......	....	......	....

the gross scores, and not only the scores as equated for errors but also the cases where the initial and final records were identical in respect to accuracy,[1] we can make a reasonable prediction concerning the reduction in time which would have occurred had the individual worked at the beginning and at the end of the practice with the same accuracy.

The ratios of such scores for the last five examples to those for the first five were as follows: 14, 20, 21, 23, 26, 28, 29, 30, 31, 32, 34, 36, 39, 42, 42, 44, 47, 48, 50, 50, 50, 50, 52, 58, 59, 60, 64, 70. The median is .42 (P. E. .02) and the median deviation from it is .10. The separate scores are subject to somewhat large variable errors so that it would be unsafe to infer much from the range of variation.

This estimate of the general amount of improvement would be very, very slightly altered by any reasonable system of equating errors and time. This can be demonstrated by actual trial of such systems and also by taking those cases where the difference in accuracy between the first and last five examples was nil or slight. For the eleven such individuals the median of the ratios of the scores of the last five examples to the corresponding ratios of the first five was .41 (P.E. .03).

The fact that these mature and competent minds improved in the course of so short a training so much as to be able to do an equal task in two-fifths of the time first taken is worthy of attention because of its bearing upon the problem of the influence of improvement in one function upon the efficiency of other functions. It is clear first that the training which this group had had for twenty odd years in remembering facts, resisting distractions and carrying in mind a complex series of relationships had left this special function of mentally multiplying a three place number by a three place number in a very easily improvable condition. Such could not have been the case if the components of that previous training had exerted each even a very moderate general influence. It is clear also that this improvement of over fifty per cent. must have been restricted closely to the special function involved. The most ardent advocate of the general influence of specific practice would not, I judge, claim that ten hours' drill in any one thing could improve an already well educated adult 50 per cent. or 5 per cent. or even 1 per cent. in the average of all his intellectual processes.

In estimating individual differences in the amount of improvement and in estimating the relations of these differences to other mental characteristics of the same individuals, the ratios listed

[1] This comparison will give only a limit, for it means for a person who improves in both speed and accuracy that a better early record than usual is compared with a worse late record than usual.

above must not be taken thoughtlessly at their face value. For a person to change from 400 seconds per example to 200 is not necessarily the same amount of improvement as for him or another to change from 200 seconds to 100 seconds. The second is probably an improvement which fewer individuals would be capable of, which the same individual would take longer to attain, and which, if analyzed into its constituents, would be found to be different from the other. To call the two equal as fractions must not lead one to infer any thoroughgoing equality in the facts which the fractions only partially represent. The relation of one-half of a man to a whole man is by no means the same as the relation of one-half of an earthworm to a whole earthworm, or of one-half of a dollar to a whole dollar. In fact, every measure of improvement by a gross difference or by a ratio must be accompanied by a statement of the initial or final gross actual ability.

It is beyond the province of this article to discuss the intricacies of methods of measuring change. The aim here is only to show and very roughly measure those differences by a method to which no one can properly object. Consider, then, the following eight records:

GROSS RECORDS

Initial (I)		Final (F)		Ratios F/I		Estimated Single F/I Ratios, errors being equated into time
Time	Errors	Time	Errors	Time	Errors	
2765	6	377	6	14	100	14
1870	2	450	1	24	8	20
2665	19	637	10	24	53	21
2130	7	600	1	28	14	26
2185	16	1803	5	82	31	70
1590	13	950	14	60	108	59
3535	12	1915	5	54	42	50
2834	23	1511	10	53	44	50

Now whether we regard a poor initial record as favorable to later improvement or not; whether we mean by twice as much improvement twice as much gross reduction in time or twice as much percentile reduction or twice as low an ending-beginning ratio—in any case we find some one of the first group who improves two and a half times as much as some one of the second group. There is, then, a range of at least two and a half to one among the 28 students on any reasonable and on

most unreasonable methods of scoring improvement. Nor would the unreliability of the measures of individual improvement be any more likely to decrease than to increase this range.

An investigation of the relationship of this difference in amount of improvement to other differences amongst the 28 individuals becomes necessarily very complex, and I shall not present the evidence here. There is a positive correlation with general intellectual achievement, a correlation which I estimate roughly as at least .4, possibly much higher.

There is apparently a zero or a slight negative relationship with the vividness and fidelity of visual images of the numbers, partial products, etc. The proportions of those of strong and of weak visual images were closely the same in those improving much and those improving little. Of the few cases who reported increase or decrease in the strength of the visual images of the numbers during the course of practice, those who reported a decrease improved somewhat more. It is also significant that more individuals reported a decrease than did an increase (9 and 3 respectively).

The Limits of Practice Effect

For the function practiced, the multiplication of a three place number by a three place number, the physiological limit is, for a capable person, very, very low, for such a person could, by devoting himself absolutely to it long enough, arrive at a knowledge of a large part of the multiplicalion table up to 999 times 999, and at an absolute knowledge of the multiplication table up to 99 times 99. There is no question of the attainment of such a final limit of practice in this experiment, but one individual (No. 4) did reach a condition beyond which the remaining practice of the experiment itself did not appreciably improve him. (See Fig. 1.) Such was possibly the case also with individual 9.

In view of the fact that the ultimate limit is far below the ability recorded by subject 4, the arrest of practice effect at this level may be taken to represent a 'plateau' from which the curve would sometime descend.

Changes in the Rate of Improvement

Practice in mental multiplication with two three place numbers is not well fitted to show changes in the rate of improvement because of the large variation in the result for any one example which a slight lapse of attention or memory may cause, though possibly it is as suitable as any equally complicated and difficult purely mental function would be. For this special purpose the presentation of the numbers themselves to sense perception, or the use of two place numbers, might be better. However, certain facts are shown with sufficient clearness and reliability.

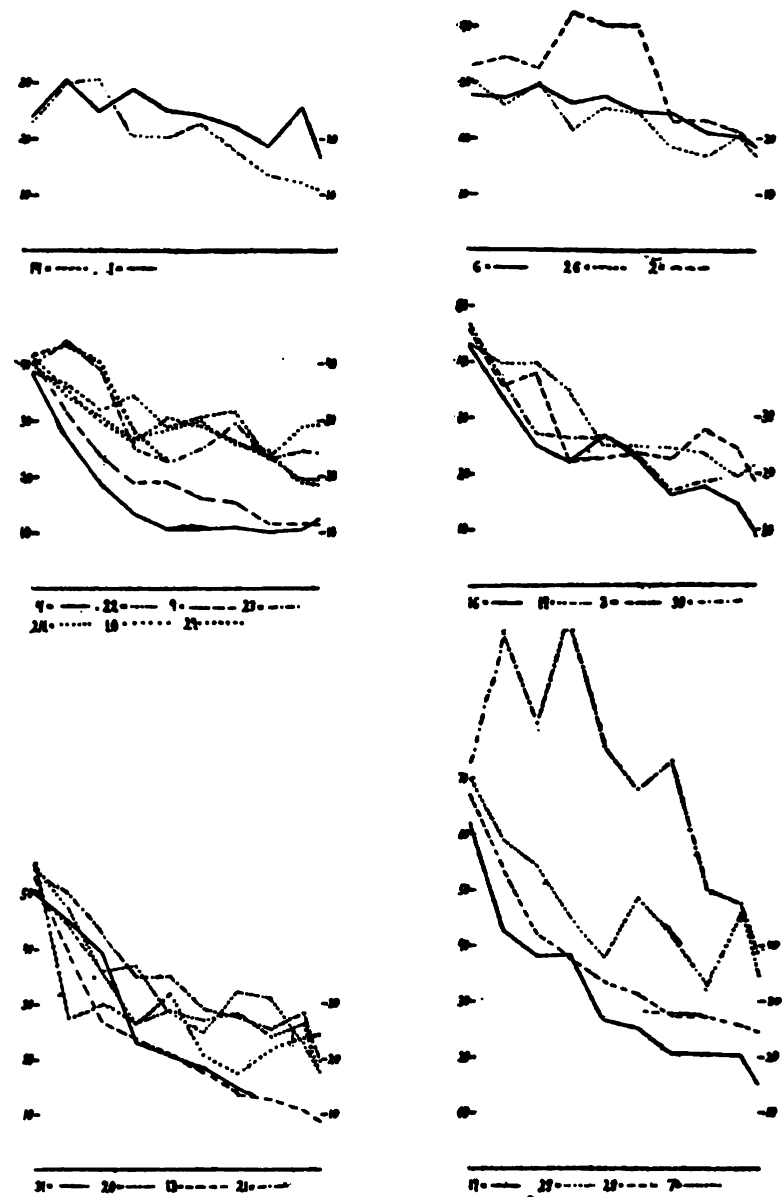

FIG. 1. The changes in the Rate of Improvement in Individuals. The course of practice runs from left to right, the whole abscissa-

length equalling 95 examples. The length of the curve represents (in hundreds of seconds) the time required to do ten examples plus the allowance for errors made. Individuals are grouped according to their degree of ability in the first ten examples. For the reasons stated on pages 375, 377 and 378 only the general sweep of each curve should be considered in arguing concerning individual differences.

In general, the earlier periods of practice show the greatest gross reduction in the scores. The graphic records of the individuals (Fig. 1) show this change.

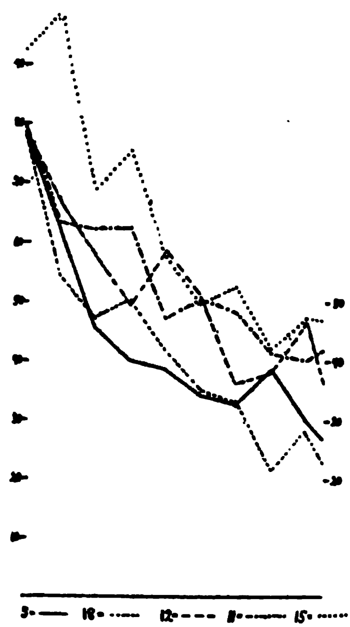

FIG. I (Continued).

The apparent changes in the rate of improvement, that is the forms of the practice curve, are widely different amongst different individuals. This, again, is clear from the graphic records. These apparent changes in the rate of improvement are due in part to the variations in conditions from which the effect of mere practice *per se* must be freed before one can prove that the law of change in the rate of improvement varies with individuals and, if so, how far it varies.

If, however, there were one law of change of rate of improvement from the start through this period of practice identical for all the 28 individuals, we could, from the present data, ascertain fairly closely what the law was. We could, that is, answer the following question:

Considering the 28 individuals as all starting at "the ability given by 10 to 20 years of general experience with mental work," and ending with 'the ability given by 10-20 years of general mental work plus the mental multiplication of 95 examples,' and considering the change in their rates of improvement from the start to the end to be due to one general law of change of rate plus individual deviations from it due to internal and external disturbing factors, what is this general law of change of rate?

The answer to this question is given by the continuous line of Fig. 2, which presents approximately the one rate of change from which the 28 separate rates of change could come with the least improbability as a result of disturbing causes. It is obtained by eliminating the total amount of change from consideration in every case by taking the *differences*—score for examples 1 to 10 minus score for examples 11 to 20, and so on up to 81 to 90, and dividing them by the total change, *i. e.*, score for 1 to 10 minus score for 81 to 90. We have, then, 28 practice curves all beginning at 100 and ending at 0, and can find the one such curve which represents the central tendency of them all.

It might well be that though no such one law held for the change of rate of improvement from the beginning to the end of the practice given in such an experiment, some one law

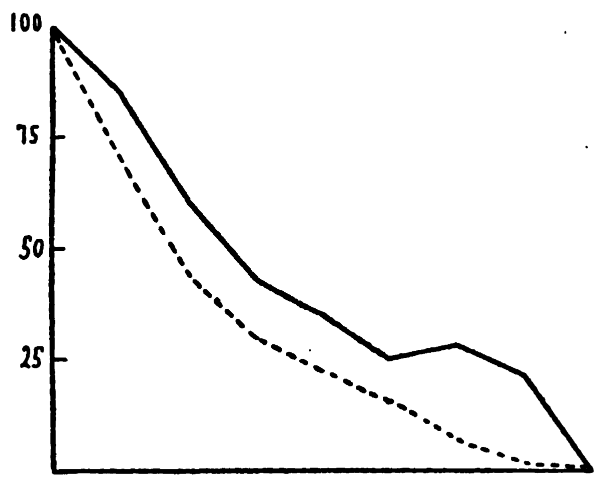

FIG. 2. The General Law of Change of Rate of Improvement in the Case of each of Two Suppositions.

might hold for this change of rate of improvement from a given ability (say to do 10 three place examples in 50 minutes

with 15 errors) to another given ability (say to do them in 25 minutes with 10 errors). That is, there might be identity in the rate of change in improvement amongst individuals whose total improvement was identical, so that significance attaches to the answer to the following hypothetical question:

Considering the changes in the rate of improvement from any one given degree of ability to any other given degree of ability to be due to one general law of change of rate plus individual deviations from it due to internal and external disturbing factors, what is this general law of change of rate?

This general law for the case of progress from a score of 4,000 to a score of 2,000 for ten examples is approximately that given by the dotted line of Fig. 2. It is obtained by taking the individuals[1] who, in some succession of tens of their practice, progressed from 4,000 or more to 2,000 or better, and plotting for each a curve irrespective of the *amount* of practice that carried them from a 4,000 score to a 2,000 score. The curves, that is, all start at 4,000, all end at 2,000, and all occupy the same length of the abscissa, so that they vary in the one element of the rate of change of improvement. The dotted line of Fig. 2 represents the one such curve from which the separate curves could be derived with the least improbability.

The reader will understand that the writer does not attempt to decide whether there is, for either case, any such one general law. As was stated on a previous page, three place mental multiplication is not a specially favorable case to study the issue and the measurement of the influence of external factors could not, in the present study, be made satisfactorily. So far as the evidence does go, it favors the conclusion that the differences amongst individuals in the changes in rate of improvement are due not only to the influence of one same law plus differences in conditions, but also to the action of radically different laws acting on different individuals according to the different physiological changes in them to which the improvement is due. The curves of Fig. 2 would then be mongrels representing no significant laws of nature.

The Influence of Equal Practice upon Individual Differences

Experiments in practice offer evidence concerning the relative importance of original nature and training in determining achievement. In so far as the differences amongst individuals in the ability at the start of the experiment are due to differences of training, they should be reduced by further training given in equal measure to all the individuals. If, on the

[1] In this case two individuals not in the 28 were included since the completion of the entire 96 examples is here irrelevant.

contrary, in spite of equal training the differences amongst individuals remain as large as ever, they are to be attributed to differences in original capacity.

As a matter of fact in this experiment the larger individual differences *increase* with equal training, showing a positive correlation of high initial ability with ability to profit by training. The data are given in Table II.

TABLE II

The ratios of the worse to the better records, early and late in the course of practice.

The numbers 1 to 28 refer to the records in order of excellence, the same number thus possibly meaning different individuals.

Records Compared	For First 5 Examples	For First 10 Examples	For Second 10 Examples	For Ninth 10 Examples	For Eighth 10 Examples	For Last 5 Examples	Relation of late to early variability, by different measures of it
21/8	1.73	1.68	1.61	1.44	1.42	1.41	Less
22/7	1.93	1.88	1.71	1.57	1.49	1.60	Less
23/6	2.21	2.04	1.92	1.78	1.59	1.95	Less
24/5	2.32	2.36	2.08	2.25	2.70	2.81	Greater
25/4	2.44	2.59	2.31	2.84	3.76	3.18	Greater
26/3	3.00	2.83	2.44	3.27	4.07	3.48	Greater
27/2	3.36	3.29	3.53	3.66	4.48	4.58	Greater
28/1	5.60	4.01	3.85	5.02	4.58	5.61	Greater

It is impossible as yet to demonstrate how far this influence of equal practice extends amongst the important mental functions, partly because common life does not make the experiment of equal practice often enough for us, and partly because comparable units for the measurement of mental achievement are so often lacking. To the author the achievements of students in schools and colleges seem to show in general that the greater original capacity gains as much or more from the same environmental training, and the differences amongst individuals who have all been brought practically to their physiological limit in the case of speed of reading, musical technique, ability in science or business or the like seem to be in general greater than the differences amongst the same individuals at earlier equivalent stages in practice. Moreover, it seems extremely probable from many facts of dynamic psychology that the man who has the capacity to improve to a given small degree more quickly than another should also improve more quickly to the next degree and should also, by and by, be capable of improving to a higher degree if given the maximum of efficient training.

THE PROBLEM OF FATIGUE.

By William H. Burnham.

Hygiene is sometimes called the science of the future. Perhaps the same is true of psychology. However that may be, its doctrines at the present time are largely in the form of problems. This is encouraging; for in the field of science a definite ignorance is the beginning of wisdom.

The whole subject of work and fatigue among school children has been studied by many investigators for twenty years or more, and the outcome of it all is a series of tolerably definite problems. The general problem of fatigue involves, on the one hand, many physical factors, on the other hand, many psychological factors, and also the relation of these two sets of factors. The relation of fatigue to the various psychic factors,—such as practice, warming up, habituation, voluntary effort, and the like, has been carefully studied by Kraepelin (8) and the complexity of the problem has been well shown by Dr. Bolton. (2.)

From the physiological side the problem of work and of fatigue has thus far been treated by most writers as if the human organism were practically a complicated and delicate machine whose working is affected by its environment as regards temperature, humidity, etc., by the overcoming of friction and its own inertia, by periods of rest to avoid over-heating of parts, by suitable oiling, cleaning, etc., but whose ultimate output of energy is determined by the fuel it consumes. The nervous system has been likened by Dr. Donaldson (3, p. 293) to an engine. The analogy is a good one for its purpose. But the psycho-physical mechanism has a power of adaptation not possessed by any machine. Especially is this true during the period of growth and development. Thus the problem for the work of children seems to be almost hopelessly complex, because within certain limits the power to resist fatigue can be acquired. Very likely as suggested by Weichardt's (19) investigations this is due in part to an acquired immunity to the toxic products of fatigue; but whatever the cause of it the fact seems to be shown.

The problem is still further complicated by the fact that the amount and quality of brain work that can be done is determined by many conditions. Among the most important of

these are the following: Race, sex, physiological age, food, drugs, sleep, temperature, light, air, season and meteorological conditions, exercise, posture, health conditions, refraction of the eyes, conditions of hearing, nasal breathing, and the whole mental attitude or apperception.

The problem of work and of fatigue in simple form is that of determining what periods of work and rest will enable an individual to do the maxium amount of work within the limits of health, whether the work be physical or mental.

It is not easy for an individual to settle this question for himself. For a group of individuals the problem is still more complex on account of the remarkable individual variations in ability which all investigations show, and for a group of individuals in the period of growth and development the complexity may fairly be called infinite. Nevertheless, it is a question that must be answered every day in the study and in the schoolroom, and so we cannot shirk it on account of its difficulty.

The problem presents itself in concrete form in testing a muscle or a group of muscles in the laboratory. One can determine just what rhythm and what alternation of work and rest will enable a group of muscles lifting a given weight to do the maximum amount of work. The problem here is not so simple, however, as it at first appears. One of the most striking results of the early experiments with the ergograph by Dr. Lombard (10) was the periodic recovery of the muscle. Contracting the muscles of his middle finger till it was no longer possible to lift the given weight, this investigator found that if he continued the effort, presently the muscles responded and the weight could be lifted nearly as high as before; and this loss and return of power occurred several times. This recovery Dr. Lombard likened to certain phenomena of periodicity shown in ordinary life, such as the second breath of the runner; and he attributed it to central causes. Recent experiments by Treves (16), however, indicate that this phenomenon of periodicity can be obtained by electrical stimulation of the muscles, and consequently it is referred by him to peripheral rather than central causes. It seems probable that the cause of this periodic recovery is to be found partly in the nerve centres and partly in the peripheral apparatus, that when it occurs under ordinary conditions of stimulation both central and peripheral causes combine to produce it. While Treves (16) has shown that these phenomena may be produced by peripheral stimulation alone, as Dr. Bergström (1) has said: "This does not, however, show that changes in force, even rhythmic changes, may not also occur in the nerve centres stimulating the muscle." Whether this recovery is due to central causes as supposed by Lombard (10), or to peripheral causes as maintained by Treves

(16), or partly to changes in the nerve centres and partly to changes in the periphery as suggested by Bergström (1); and whether the physiological causes are changes in nutrition, or the results of an antitoxin, or some unknown factor, the phenomenon suggests the possibility of extending the limits of the functional activity of a set of muscles and even of one's working capacity in general. But how far and in what way it may be safe to attempt this, we do not know.

At all events the factor of training must be reckoned with, and this in itself presents a very complex problem. The difference between the trained muscle and the untrained muscle is not merely a difference in size and nutrition. Professor Hough (5) has shown that in case of the untrained muscle the fribrillæ are lacerated by exercise—hence the soreness that results. In the trained muscle this tearing does not occur. Also a very complex process of adaptation or habituation is involved. To quote Dr. Bergström (1, p. 273) again: "A certain class of psychic factors certainly affect the records; competition and encouragement often greatly augment both momentary strength and endurance, and discouragement or a sense of failure may produce the contrary effect. By pretending to present a subject with a series of increasing loads, successive reductions in the record may sometimes be observed, while as a matter of fact the load remains constant. Habits of effort, like habits of sleep, may no doubt exist or be established by training, and even variations like the Lombard curves are not impossible from such a source, but might be due to overcoming a reflex tendency to rest, just as we may by persistent effort counteract a tendency to fall asleep." Even on the physical side the problem is a complex one. The problem of training is closely related to that of immunity; and it will be helpful briefly to consider the latter.

When certain poisons are introduced into the animal body, nature at once proceeds to manufacture an antidote, and if the amount of poison introduced at any one time is not too large, the antidote is usually sufficient to correct its effect. This has been illustrated very vividly in modern therapeutics.

The modern discovery of antigenes and anti-bodies has revolutionized the treatment of many diseases. Antigenes are substances of unknown chemical composition which act as stimuli for the production of anti-bodies; and anti-bodies, not to attempt a technical definition, are substances which act as antidotes to certain poisons, or as checking agents upon the antigenes. The toxin and antitoxin of diphtheria furnish the standard illustration. If the toxin of diphtheria is repeatedly injected in proper quantities into the blood of a horse, this stimulates the production of an anti-body, which can be taken from the veins

of the horse and used as a remedy. Thus is produced the antitoxin for diphtheria, which during the last dozen years has so enormously reduced the mortality from this disease.

In a similar way are produced antitoxins for a number of diseases: for tetanus, for dysentery, for the poison of snakes, etc. But it is not only in disease that such anti-bodies are produced (19), and not merely the poisons that act as antigenes in producing these bodies. Antiurease, antitrypsin, antipepsin, and the like, belong to this same group of anti-bodies occurring in the normal animal.

In tuberculosis we have an interesting illustration, apparently, of the general law of immunity. It is estimated that one-seventh of the total mortality results from this disease; but a vastly larger number have been afflicted with it. Dr. Flexner estimates that at least 90 per cent. of the total population have been subject at one time or another to tuberculosis. This has been shown by the results of innumerable autopsies. The resistance to the disease, however, is so great in most people that it has been cured or arrested. Again, the most successful experiments in the treatment of tuberculosis in animals have been those of direct inoculation with tubercle bacilli, thus developing immunity,

Acquired resistance to fatigue seems to be also a case of immunity. It was apparently proved by Mosso that toxic products result from the functional activity of the muscles and that these are the chief cause of fatigue. Thus when the blood of a tired dog was injected into the veins of a normal one the latter showed the symptoms of fatigue. If Weichardt's studies (18 and 19) are to be credited this is only half the story. His results not only corroborate Mosso's theory of a fatigue toxin, but have also shown apparently the existence of certain anti-bodies that produce an antitoxin of fatigue. From his remarkable experiments with mice this investigator reports that when a mouse was worked to exhaustion he found a toxin produced in the muscle serum. This purified from salts, creatin, urea, and albumen, and injected into another mouse caused fatigue, and in large doses death. When a horse was treated with frequent injections of this purified serum an antitoxin was formed which acted as an antidote to fatigue.

Weichardt (19) has made many experiments with this toxin and antitoxin of fatigue. With the artificially produced fatigue toxin all the characteristic symptoms of fatigue, even death from exhaustion, are produced. On the other hand, he has also succeeded in neutralizing the effect of the fatigue toxin by means of the artificially produced antitoxin.

The discovery of fatigue toxin in the excreta of the body and even in human urine shows that the formation of toxic

products occurs in ordinary physiological fatigue, and that extreme pathological fatigue is not necessary to produce this toxin. The antitoxin also, not only may be produced artificially in the manner just mentioned but is produced during normal functional activity. With the appearance of moderate quantities of the products of fatigue there always occurs in the healthy organism an increased formation of the specific antitoxin, *i. e.*, there is always the tendency to develop immunity to fatigue.

This has been shown by Weichardt (19) in his experiments with mice. The normal curve of work for a mouse is similar to that of a man as shown by the ergograph. If the mouse is treated with an injection of a moderate dose of toxin the curve of work is somewhat raised, and then sinks only slowly after a prolonged period. The reason for this is that a moderate dose of the fatigue toxin works as an antigene and produces the antitoxin in increased quantity, thus enabling the animal to do more work. If, however, the mouse is given a large dose, so that the cells which produce the antitoxin are injured, the ability to work is decreased; or, if the dose be sufficiently large, the animal soon dies from the effects of it. If, however, mice which beforehand have been treated with the antitoxin are given a large dose of toxin a continuous curve of work is produced. The curves of men, who have been trained in the use of the ergograph, correspond to the curves of mice treated with the antitoxin. Of course, the human subject cannot be treated with a large dose of the toxin; but the stages of increased ability to work, *i. e.*, the stages of active immunity brought about by the stimulus of small doses of the toxin, are shown. In other words, the process of training is not a mere process of modification and increase of muscle tissue, it is also a process of rendering the subject immune to fatigue. In the trained subject the ability to work is increased as a result of increased power to resist fatigue.

One is at first inclined to think that Weichardt's (19) study is another case to be added to the long line of discredited investigations in this field. But whether his results stand the test of further experimentation or not, the problem of fatigue is not misrepresented by them. It is in part the problem of adaptation or immunity. And for the present we may naturally conceive of this as analogous to the immunizing contest in infection as represented by the hypothesis of Dr. Welch:[1] "The struggle between the bacteria and the body cells in infections may be conceived as an immunizing contest in which each participant is stimulated by its opponent to the production of

[1] As cited by Ricketts in "Infection Immunity and Serum Therapy". Chicago, 1906, p. 567.

cytotoxins hostile to the other and thereby endeavors to make itself immune against its antagonist."

Of course, all of this applies primarily to muscular fatigue, but it is probably true that the conditions of fatigue resulting from mental activity are similar and that within certain limits immunity to fatigue may be developed, or in psychological terms, habits of increased ability to work may be formed. This was pointed out some years ago by German writers. Dr. Erb (4, pp. 26-27) for example writes:

"The nervous system in its comprehensive mode of habit and adaptation is capable of increased exertions as well as able to endure greater injuries, as von Ziemssen recently has shown in an interesting manner. . . . Just as our senses become accustomed to all possible impressions so that they pass almost unnoticed, so also the brain will gradually learn to accustom itself to all the daily injuries which beset it, to the noise, the hurry, and unrest, the mechanical shocks in our everyday life and business. It will be more difficult to be sure with the psychic shocks, the excitements and emotions, but even here, habit, training of character, and self-control can certainly do very much; and thus we have good prospect that our nervous system will adapt itself, to a certain degree, to the demands placed upon it by our culture of to-day and by habituation neutralize their injurious effects."

The relation of the mental factor to fatigue likewise presents a very complex problem. It has been briefly stated by Dr. Seashore as: "The correlation of psychological and underlying factors, such as physical, chemical, histological, and electrical phenomena. If the attention wave varies with fatigue, as Prof. Pillsbury has demonstrated, we may ask, what are the physiological factors which condition that variation? What feature in the mental work is it that causes the physiological state? What chemical processes may be traced? What is the cell modification? Are there any characteristic electrical variations?"

Again we have the great problem of the relation of one kind of fatigue to another and whether there is one reservoir of energy or several. It has frequently been noted that change of occupation is a means of rest. The child tired with school work turns with enthusiasm to his play; and even a game requiring close attention or an interesting book is taken up with a zest which indicates that the interesting occupation is rest from the routine of study; and the man of business weary with the work of the day turns at night to the study of some engrossing fad and finds it a means of rest and recreation. Mosso (11, p. 120) in his book on fatigue says: "Apparently the fatigue is localized only in a certain region of the brain; for one often sees

persons who have become incapable of thinking about a certain subject or considering a matter of business find recreation in thinking of something quite different, or even rid themselves of a feeling of pressure in the head by turning their attention sharply to other things of a different character, for example, to chess playing."

Weygandt (20) working in Kraepelin's laboratory, has studied this question. His problem was this: does change have a favorable or unfavorable effect upon the amount of work done? He employed Kraepelin's method of adding varied with several other forms of activity,—learning by heart of numbers and nonsense syllables, the search for a definite letter in a connected text, reading of a foreign text of different kinds, —Latin, Italian, Hungarian, Hebrew, the writing down of a known series of letters, and finally, a series of experiments with complete pauses instead of change of work. Almost all these methods were used for the chief work as well as for the change of work. Of the different methods, adding predominately appeals to the associative processes; the learning of numbers and syllables to memory; the reading methods involve perception and assimilation. First, he worked for one day at a definite form of activity for one hour and a quarter; then on the next day he worked for the first half-hour at the same form of activity, this determining the character of the day's work; then followed a change taking up another definite activity, for half an hour; then again for fifteen minutes he worked at the original activity to determine the effect of a change on the amount of work done. These experiments extended over some 97 days of experiment, the usual control of conditions being adopted.

As a result of these experiments, it appears that change of the method of work is not under all circumstances favorable. Often the result is positive, often negative, but usually only very slight. Second, the difficulty of the work appeared as a determining factor in the result. A form of work broken by a more difficult form of work shows thereafter less results than would be expected; broken by a lighter form of work, on the other hand, shows better results. Third, it is all the same whether the mental work that alternates with another is of similar or dissimilar character. In a word, the result of his investigation furnishes no evidence for the assumption of a partial and localized mental fatigue. And Kraepelin (8) interprets his results as indicating that fatigue is general. "Fatigue through mental work is," he writes, "so far as we know, a general fatigue. As especially Weygandt's study on the effect of change in mental work has shown, the fatigue through a particular activity also reduces the capacity for such work as is brought about

through quite different mental activities. Thus the necessity of rest and sleep arises at certain times regardless of whether the same or changing work has provoked it. Only the difficulty and not the kind of mental work is significant for the general extent of fatigue."

Dr. Seashore thinks that Kraepelin is wrong in this and he reports experiments in Iowa laboratories that have demonstrated that the kind and degree of fatigue both depend upon the kind as well as the degree of mental exertion. It seems to be still a problem whether there is one reservoir of nervous energy or many reservoirs, or in other words whether fatigue is always general fatigue, one and the same, or whether it is often local fatigue. There is some reason for believing that there may be a difference between children and adults in this respect, one reservoir representing more nearly the fact for the unorganized system of childhood.

It would seem at least that the popular idea that change of work means rest, and the pedagogical maxim emphasized by Richter (13) as the result of his experiments with school children in occupations similar to those of the ordinary school work, must be modified. The fact probably is that in the more ordinary cases it is not fatigue which is appreciably lessened by change of work but rather the feeling of boredom, the ennui, the *Müdigkeit* not the *Ermüdung*, as Kraepelin (8) distinguishes the two. In the more extreme cases, as illustrated in the case of the exhausted soldier who arouses himself to renewed activity in a crisis, or the experimenter in the laboratory who exhibits the phenomena of recovery, or the runner who gets his second breath,—what happens is probably that a new store of energy is in some way set free; and, although the feeling of fatigue has disappeared, fatigue itself may have been increased and the ultimate ability of the organism to work diminished. But definite results are lacking and more experimentation is needed.

Professor James (6) has recently studied many of the phenomena similar to those of second breath, and concludes that our organism "has stored up reserves of energy that are ordinarily not called upon, but that they may be called upon: deeper and deeper strata of combustible or explosible material, discontinously arranged, but ready for use by any one who probes so deep, and repairing themselves by rest as well as do the superficial strata. Most of us continue living unnecessarily near our surface."

He reports many interesting cases where old habits of performance have been broken up and an astonishingly increased capacity for work developed. Among the dynamogenic agents that he has studied are the training of the Yoga system, Christian Science, and the like, religious, political, philo-

sophical, and scientific conversions, and among more concrete energy releasing ideas, "Fatherland," "the flag," "the Union," "Holy Church," "the Monroe Doctrine," "Truth," "Science," "Liberty," Garibaldi's phrase "Rome or Death," etc.

This investigation opens the deeper problems of neural economy. That there are many such cases as those cited by Dr. James (6) cannot be doubted. Further study may show that such cases fall into widely different classes. Some of these are probably pathological. An abnormal activity is aroused, the subject may perhaps kill himself or become a wreck in a few years. Most of them, however, are perhaps normal and, as suggested by Professor James (6), they frequently represent the cure of nervous disorder. In some cases the great improvement in the individual's productive activity may be brought about merely by stopping waste. It is astonishing how much this amounts to in the case of many persons. Every one in his experience has the opportunity to note the great economy of co-ordinated activity and the extreme waste when confused, or nervous, or "rattled." Some persons work without waste excluding all interference of association and all unessential movements when under pressure. Others are made nervous by undue pressure and work most economically when at leisure. When hard pressed they are nervous and confused, when at ease they make no false motions, have no interference of association, and work directly and economically. In both classes the great improvement in productive activity is brought about by the same means, namely, the stopping of waste.

In other cases the great improvement in productive activity may be due to an improvement in nutrition, more energy is stored up, and thus the amount of energy available for work is increased. This improvement may be brought about in two ways: First, the checking of worry and habits of emotional prodigality by working under a strong stimulus is quite enough in many cases greatly to modify and improve, it may be, all the nutritive functions of the body; and this in some cases may be the chief factor.

Again, the nutrition may be greatly improved perhaps in a very different way. It seems probable that the nervous system functions rhythmically; first, a period for the storing up of energy, a period when the anabolic processes predominate; then a period of explosion when the katabolic processes predominate, and so on. Now as is generally recognized a suitable period for the storing of energy is essential in order to give the necessary conditions for the vigorous explosion of energy. It is probably also true, although this seems to have been neglected, that the vigorous explosion of energy is likewise a necessary condition for the proper functioning of the anabolic

processes that predominate in a subsequent period of rest. Verworn's (17) hypothesis that after exercise there is not only the restoration of what has been lost but an over-compensation is in harmony with this view.

Thus in some cases the improvement in nutrition is probably caused by the fact that the vigorous explosion of energy due to a strong stimulus has furnished the one essential condition for the functioning of the nutritive processes on a higher level. Thus in certain conditions excitement and the explosion of energy may be as essential as rest and the storing of energy.

Again, this theory is in harmony with the results found by Professor Patrick (12) in his study of sleep, referred to by Dr. James (6). After one subject had been kept awake for over 90 hours, 13 hours of sleep seemed to be enough to restore him to normal condition. This was explained in part by Professor Patrick on the theory that the rate of repair was increased, the anabolic processes were so stimulated that the short period of sleep was all that was needed. In these experiments the essential condition of the increased nutrition seemed to be the increased expenditure that had preceded. In ordinary sleep also we have an illustration of the same phenomenon. Frequently the condition of sound and refreshing sleep seems to be the normal exhaustion that results from vigorous activity during the day, a fact that has been observed since the days of Solomon: "The sleep of a laboring man is sweet whether he eat little or much."

Another factor which probably accounts for some of the increase of work in such cases is the development of immunity to fatigue, perhaps to the toxic products of fatigue, as suggested by Weichardt's (19) experiments.

In certain instances, perhaps in most, all of these factors combine to produce the increased efficiency of work. In most of the cases probably a better habit of functioning is the secret, but this involves better nutrition and very likely the acquisition of immunity.

The study of the neurasthenic is instructive. The characteristic symptoms are too familiar. They are chronic fatigue, inability to perform any continuous task, lack of energy for any vigorous action, and also irritability, restlessness and a continuous dribbling out of energy. If the hypothesis already suggested be true, then this condition may be due to any one of three causes.

First. The individual may never have developed any considerable amount of nervous energy. He may be one of those, as suggested by Tissié (15), who are born in a condition of fatigue. The nervous system may be undeveloped so far as its trophic functions are concerned.

Second. The individual may have a nervous system that is normal, but may never have been stimulated to any vigorous explosion of energy, consequently a necessary condition for the proper storage of energy is lacking. Such probably, as already suggested, are some of the cases cited by Dr. James (6), and the remarkable results of the Yoga system, the emotional stress of conversion, or of falling in love, or what not, are due to the fact that thus a vigorous expenditure of energy was caused and this provided the one essential condition lacking for the normal storing of energy. In such cases we may conclude that a strong stimulus or shock of some kind is the thing needed.

Third. In another class of cases the individual has seriously over-taxed himself. The nerve cells have perhaps actually been injured so that normal recovery is temporarily at least impossible. In such cases what is needed first of all is rest. To ignore this condition and attempt to cure by further work, by greater stimuli and the like, would be useless and perhaps fatal.

We are not concerned here directly with the practical questions of therapeutics, but we note that we may have practically the same symptoms with patients of very different nervous conditions and requiring diverse treatment. The practical problem of diagnosis in such cases is no more complex than the hygienic and economic problem of determining the proper quantity and quality of activity. One must know the condition of an individual's nervous system if one is to prescribe a method of work.

Thus the problem of tapping the deeper levels of energy involves the problems of nutrition, of physical and mental adaptation, and of immunity.

Among other important questions which still remain open in this field, are the following:

First. Although it has been shown that certain phenomena like periodicity may be brought about by peripheral causes alone, the question is still unsettled whether the fatigue resulting from ordinary neuro-muscular work is primarily central or peripheral.

Second. It does not seem to be as yet satisfactorily settled whether the nerve centre or the periphery is better able to resist fatigue, although it seems probable that the nerve centre has greater resistance.

Mlle. Joteyko (7) infers from her experiments that there is a hierarchy in the tissues as regards their resistance to fatigue. The nerve trunks appear to have greater resistance according to the experiments of a number of writers. According to Mlle. Joteyko's (7) experiments the reflex centres of the cord

have greater power of resistance to fatigue than the psychomotor centres, and both have greater resistance than the peripheral apparatus. Thus she concludes that in physiological conditions the phenomena of motor fatigue are due to the arrest of function of the intra-muscular nerve endings.

Thus Mlle. Joteyko (7) thinks that she has shown that the origin of fatigue is in the main peripheral and that fatigue plays a kine-philetic rôle, that is, it is a defensive function of movement. One feels inclined to accept the results of this brilliant experimenter; but further investigation of this point is desirable.

Third. The question of the range of individual variation in ability to work and to resist fatigue, and of types of workers, and the like, is suggested as an important one by the studies of Kraepelin (8) and Mlle. Joteyko (7).

Fourth. The problem still remains of finding a good practical test of the psychophysic condition of an individual that can be used by a teacher or a physician to determine whether one is overworked or not. Perhaps it is too much to expect that any simple test of a condition so complex will ever be possible.

Thus the whole problem of fatigue is an infinitely complex one. We are not concerned here with practical inferences from the investigations that have been made. We may add a few words, however, in regard to the relation of the problem to pedagogy. With our present knowledge it is impossible to give an adequate solution of the practical questions involved. One general point, however, is emphasized by the results of these investigations. Immunity to the toxic products of fatigue is probably brought about by gradual habituation to small doses of fatigue toxin. Large doses decrease the ability to work and may do permanent injury. Thus it would seem that the general rule of hygiene is established in correspondence with that resulting from ordinary observation, that the way to develop power to resist fatigue in children is by short periods of intense work followed by periods of rest rather than by prolonged periods of work; and in fact that the latter should be looked upon with grave suspicion as likely permanently to injure the ability to work.

This inference is largely in harmony with the suggestions that come from the observation of cases of the character reported by Dr. James (6). As he suggests from a very different point of view habit and adaptation in conflict with the excitement of emotion are determining factors. "The emotions and excitements due to usual situations are the usual inciters of the will. But these act discontinuously; and in the intervals the shallower levels of life tend to close in and shut

us off. Accordingly the best practical knowers of the human soul have invented the thing known as methodical ascetic discipline to keep the deeper levels constantly in reach. Beginning with easy tasks, passing to harder ones, and exercising day by day, it is, I believe, admitted that disciples of asceticism can reach very high levels of freedom and power of will."

The concrete problems of the schoolroom related to the general problem of fatigue are legion. Teachers may naturally be sceptical of any attempt to solve them by scientific methods. Dr. Lehmann (9, p. 297), a gymnasium teacher, for example writes:

"An exact theory of instruction or of its separate factors of the programme, for example, is impossible because the practical work of teaching must every moment reckon with forces which experiment can neither exclude nor estimate. The pupils are not even for a moment merely heads with the angel wings of the intellect. The personality of the teacher, for example, and the relation of the pupils to him constantly influences the tension and the kind of association of the class. There are not only teachers who fatigue their pupils quicker than others, but also those whose force of suggestion holds them quite in their own power and causes them to overcome fatigue for a relatively long time. But if one seeks to estimate the personality of the individual teacher or the character of the pupils by making the experiment with a very large number and accordingly gets very large figures, the results obtained may still be of value for theoretical psychology, but for pedagogy and for the practical work of instruction they are worthless."

While this distinguished German teacher is right that no exact rules can be developed in this field, he is not to be understood as deprecating the scientific study of the conditions of school work. And probably the value of such study in emphasizing the complexity of the problem, in making definite the knowledge we already have, and in mediating between conflicting opinions, will amply reward investigation. Moreover, in such problems we cannot expect exact information, but the writer believes that more definite practical contributions than most teachers imagine are possible.

At present we are just beginning to see the complexity and the significance of the concrete problems of economy and hygiene in school work. Questions of physiological age, seasonal variation in growth and energy, individual variations in ability to work and to resist fatigue, the conditions of training and of immunity, of sanitary and of social environment, are all involved; and many special studies must be made before definite rules for the optimum conditions of school work can be formulated. And until further investigations have been made

it appears desirable that the practical questions in regard to the period of study, and the like, be solved tentatively, and that different plans be tried experimentally to the end that ultimately a solution based upon scientific data may be obtained.

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PSYCHOLOGICAL LITERATURE.

Essays Philosophical and Psychological: in Honor of William James. By his Colleagues at Columbia University. New York, Longmans, Green & Co., 1908. pp. viii, 610.

This handsomely appointed volume — generous in bulk, but refreshingly light in the hand — contains a series of nineteen essays, classified by the editors as philosophical and psychological, which have been written in honor of Professor James and pragmatism by present or recent members of the Departments of Philosophy and Psychology at Columbia University. An excellent portrait of Professor James serves as frontispiece.

The philosophical essays, which we can here only enumerate, are as follows: "The New Realism," by G. S. Fullerton; "Does Reality Possess Practical Character?" by J. Dewey; "A Factor in the Genesis of Idealism," by W. T. Bush; "Consciousness a Form of Energy," by W. P. Montague; "Perception and Epistemology," by F. J. E. Woodbridge; "Substitutionalism," by C. A. Strong; "World-Pictures," by W. B. Pitkin; "Naïve Realism: What Is It?" by D. S. Miller; "Kant and the English Platonists," by A. O. Lovejoy; "A Critique of Kant's Ethics," by F. Adler (reprinted from *Mind*, April, 1902); "The Abuse of Abstraction in Ethics," by H. G. Lord; "Purposive Consistency, the Outline of a Classification of Values," by G. A. Tawney; "The Problem of Method in Mathematics and Philosophy," by H. C. Brown. As will be seen from the titles, and as appears from the contents, these papers have fully as much to do with metapsychology as with metaphysics: a natural and fitting state of affairs, in view of the purpose of the book. As is also natural, seeing that (with the one exception noted above) they have been prepared in the course of a single year, the essays are of very various degrees of merit, ranging from the occasional contribution to systematic and historical investigation of permanent value.

The first of the psychological essays, by K. Gordon, deals with "Pragmatism in Æsthetics." Pragmatism is the disposition to look for final explanations in terms of purpose, and for reality in experienced satisfactions. Approaching æsthetics from this point of view, Miss Gordon finds that the motive of the artist is to "impose his experience on others," "to establish and preserve whatever novel emotions or supreme moments his experience may have held." "The work of the artist is to objectify new or striking emotions;" "there is always a social reference in this desire for expression." Passing from artist to admirer of art, she discovers "the characteristics of the æsthetic consciousness" in "a remoteness from immediate self interests, a suggestible and imitative attitude towards the object, and an allegiance to a reality felt to be independent of oneself." Behind all this, the keynote to the consciousness both of artist and of observer is "an instinct for excitement, a curiosity about the untried, and a liking for whatever is novel and stirring." Finally, the art-product has to be explained, as middle term between producer and admirer. Individually, emotion arises from conflict of impulses; "the objectification of emotion is an objectification of conflict, and whoever preserves an emotion preserves an ungratified impulse, a problem unsolved or a

purpose unfulfilled." Socially, the function of art is originally instrumental, "not so much a stilling as a prodding of the will." Modern art is also instrumental, only that it stimulates not to a specific but to a generic act, "and leaves each individual to live out for himself the meaning and the end of it." The term 'disinterestedness' is misleading; only some interests, by no means all, are in abeyance in æsthetic contemplation. The æsthetic moment derives its value from a purpose, and is not literally 'free from desire.' The feeling of freedom derives from the nature of emotion, as representing a choice of possible action. Art, in a word, "preserves and presents meanings at their emotional stage, before they have become explicit, definite, or solved." As stimulative and instrumental, it is "prior to that which it effects. Life and nature are in a vital sense experienced as products of art."

We have given the above summary so far as possible in the author's own words, because the paper is merely an outline sketch of a comprehensive theory. How stable that theory would prove to be, if fully worked out, we do not know. Obvious points of criticism are: the total neglect of the technical motive in æsthetic production; the psychological account of such an emotion as anger as a 'conflict of impulses'; the over-direct passage from genesis to analysis, from the question of origins to the function of art in modern society; the extreme poverty and the personally motived selection of the illustrations; and the casual and, so to say, contextless reference to other theories. It is only fair to add that some of these defects are rendered inevitable by the limits of the paper. Others, however, are not.

The next paper, by R. S. Woodworth, on "The Consciousness of Relation," falls into line with a fairly long series of recent psychological investigations, a series that extends from Marbe and Ach to Bühler. It deals with one phase of a problem which appears elsewhere as that of the 'general attributes of sensation' (Ebbinghaus), of *Bewusstheit*, *Bewusstseinslage*, *Gedankenelement*, or again, in other connections, as that of *Gestaltqualität*, *Bekanntheitsqualität*, *Wirkungsakzent*, etc. Professor Woodworth makes no reference to previous wrokers at this and at cognate problems, even in a footnote. His thesis is, in general, that there are, "in thinking, moments bare of recoguizable imagery, containing no sensations of interest — moments, nevertheless, of mental alertness and of keen consciousness," and that "it is ultra-parsimonious for the psychologist to try to keep house without these facts." More particularly, he argues for the existence and the qualitative simplicity of 'feelings of relationship,'— quoting here James's "Stream of Thought," but not quoting Herbert Spencer or Schrader. The logical bifurcation of a relation presents no difficulty either to psychology or to brain physiology. Logical complication is not paralleled by introspective complexity: "a relation may be of any order or power, and still have in consciousness a felt quality which is equally simple." Relations are, in reality, no more 'private' or incommunicable than are sensory qualities. While, however, all science deals with relations, and these or the adjustments to them carry thought forward, "the utility of the *feelings* of relation is by no means evident." Still, we cannot either say what is the utility of the particular sense-qualities; so that the aporia is, at least, old and familiar.

In comment upon this position, it may be remarked that there will always be, in a growing science, two types of workers, those who prefer to work within fields and by methods that have already proved fruitful, and those who are impatient of restriction and tend naturally to the unexplored and undiscussed. The besetting sin of the former is

overhasty and overstrict systematization. The besetting sin of the latter is incomplete and loose-ended observation, and consequent unfounded generalization. Each type must keep critical watch upon the other, but each must also try to sympathize with the other's point of view. There is, now, no doubt as to the facts to which Professor Woodworth and psychologists of his kidney appeal; the question at issue is a question of interpretation. Is 'imageless thought,' *Bewusstseinslage*, a complex of residua of images of the higher senses, of organic nascencies, of verbal short-cuts, of psychophysical sets and dispositions,—or are we in presence of what Titchener would call a structural element of consciousness? Very similar questions are to be met with in the current psychology of feeling; and there, as here, positive answer is impossible. Conservatism, however, would seem to be indicated. Let us remember, for example, how insistently the moralists have urged that conscience is a unique and irreducible mental phenomenon, and how successfully psychology has held out against the claim. Let us remember that every content of consciousness is, in a certain sense, unique and simple; my idea of the cosmos is, in a way, as unanalyzable as Professor Woodworth's 'sky-scraper' of piled relations. As reproductive unit, as vehicle of a single meaning, every idea is thus simple. Let us remember, too, the errors into which the psychologists of associationism were led by their translation of unitary reproductive function into structural simplicity. And let us remember, finally, that the establishment of a novel element of mind must be a matter of extreme methodical care, and that 'rule of three' introspections—valuable enough in themselves, as roughly mapping out the territory to be explored—cannot at all be considered as demonstrative. The fields of belief, of desire, of imagination, of volition, of thought have as yet been very inadequately surveyed by experimental means. Pioneer work is necessary and welcome; but the pioneer results must be taken for what they are, gross and tentative only; to set them by the side of verified analytical data would be illogical, as well as historically unjustifiable. Grateful for the discoveries made, we must suspend judgment upon their ultimate character. As to Professor Woodworth's appeal to cerebral physiology, that seems to us to lead nowhere. We know so little of the psychophysical processes that all sorts of constructions are possible, and all must remain unconvincing.

The next two papers, those on the Variability of Individual Judgment by F. L. Wells and on the Validity of Judgments of Character by N. Norsworthy, are of a statistical character. Dr. Wells has studied "the variability in three classes of judgment; first, the highly subjective feeling of preference for different sorts of pictures [we may interject here that Dr. Wells is not the psychological discoverer of the souvenir postal card]; second, the more objective judgment of color differences, and, finally, a type of judgment whose accuracy could be readily measured by objective means. It has appeared that in the first class the judgments of each individual cluster about a mean which is true for that individual only, and which varies from that of any other individual more than twice as much as its own judgments vary from it; that in the second class, with the colors, the variability of the successive judgments and those by different individuals markedly approached each other, but still preserved a significant difference; while in the third class, with the weights, we found that there might be even an excess of the individual variability over the 'social.' This comparison seems to afford, to a certain extent, a quantitative criterion of the subjective." Further, there appears in objective fields a phenomenon of individual tendency, of 'sensation habit,' which

needs further and special investigation. Miss Norsworthy shows first that, given impartiality of the group of judges (to be attained, practically, by random selection among the competent and trained), judgments of character have objective validity. More than that, the order of excellence in the various traits estimated is, within wide limits, reliable. These results suggest the question whether the ability to judge character is itself a measurable power; the answer is, roughly, in the affirmative. The author finds, also, that there are persons about whose character there is much greater difference of opinion than is the case with others, and that, similarly, there are traits about which there is less agreement than is the case with others. Unfortunately, there is most divergence (among the traits examined) in the judgments of integrity, kindliness and refinement,—the very points upon which emphasis is laid in the employment-blanks of the educational agencies.

The fifth essay, by J. McK. Cattell, is entitled "Reactions and Perceptions." Professor Cattell argues that the 'force and liveliness' of the perception as compared with the image or idea depend upon the greater prevalence and definiteness of the motor elements. "Images and perceptions are equally the result of brain changes, which are themselves part of the world's material system. But the brain changes which are excited from within are less likely to result in motor discharges than those which form parts of sensori-motor arcs. This is necessary if the organism is to survive and prosper." In general, "the character and validity of our perceptions are prescribed by the motor responses no less than by the incoming currents." These responses may be inhibitions, which "are as integral parts of the motor processes as discharges;" "consciousness is related to inhibition in a peculiarly intimate fashion." Sight and kinæsthesis are mainly responsible for our material and spatial world. In sight, the movements of eyes, head and body are of extreme importance. In kinæsthesis it is difficult to distinguish between image and actual movement or partial inhibition of movement. This difficulty of discrimination supports the view that in vision and audition it is the motor element that differentiates perception from idea. As "the incoming currents and the discharges which lead to definite muscular reactions give reality to the perceptions", so "the incoming currents and the discharges to the inner organs with the vaguer muscular contractions give rise to the emotions." Images and perceptions are confused only when the motor reactions are confused, when they are inhibited or are excessive.

It is not quite easy to follow Professor Cattell's argument in detail. The difference between image and perception seems, on the whole, to be a matter only of degree; the "more pronounced motor elements" in perception correspond to a "superior" vividness. But, if this is the case, why is it "only a mad cat that may jump at an imaginary mouse?" Why should not a perfectly sane cat jump a little way towards the imaginary mouse, and jump all the way towards a seen mouse? The cat appears to find a difference in kind, even in Professor Cattell's pages; for the muscular tension that it shows while watching for its prey is explicitly referred to "the vivid *perception* of the approaching mouse." (For 'approaching mouse' we may here probably read 'mouse-hole' or 'smell of mouse'.) And there is another difficulty. Incoming currents are said to "arouse the suitable responses." And centrally excited processes cannot excite the same responses as peripherally excited, since "in that case the animal would not survive." This, however, is a teleological, not a causal argument. How does the hitching-on of the suitable responses take place? If, as Pro-

fessor Cattell implies, the image and the perception would be identical but for the difference in motor elements, the incoming processes must be transferred to motor paths from the same sensory centre. How does this centre discriminate the centrally from the peripherally aroused excitations? Not, of course, by any psychological equivalent of their vividness, for the difference of vividness is produced by the difference of motor elements: how then? Professor Cattell's theory can hardly be regarded as complete until an answer to this question is forthcoming. Apart, however, from these and other points of detail, the importance of the paper appears to lie in the writer's modification of the dynamogenic theory of consciousness. If he does not deny outright the statement that "all consciousness is motor," he at least makes the image-consciousness so little motor that motor reactions of the ordinary type render it abnormal, and its bearer incapable of survival. This is welcome recognition of the restricted sphere of the motor element in the mental life, and of the modest place that is accordingly to be assigned to it in psychological theory.

The concluding essay of the volume, by E. L. Thorndike, proposes a Pragmatic Substitute for Free Will. Professor James has said that the only issue of consequence in the free-will controversy is meliorism, for which indeterminism gives possibility. Professor Thorndike seeks to show that the "natural constitution of the world makes meliorism possible and, in fact, necessary." While compatible with indeterminism, meliorism is also compatible with, and even assured by, a scientific determinism.

The first and most essential part of the argument is the proof that "the behavior of a man to the same situation becomes, as a rule, more and more productive of the satisfying and less of the discomforting as the situation recurs." Ordinarily, this proposition is taken for granted. The physiological mechanism is, however, not easy to understand; and as the whole fate of scientific meliorism hinges on it (the success of individual men implying, as will be later shown, an increase in satisfaction for the whole contemporaneous group), the author works out a hypothetical mechanism, in terms of the neurone theory. This he illustrates by reference to concrete cases, and compares with the simpler mechanism proposed by Jennings. He then passes to the second stage of the argument: the proof that "behavior in favor of the satisfying to an individual leads to meliorism for the species." The lines of proof are two: "behavior not only changes itself for the better for the same situation, but also changes the situations themselves for the better for the same organism;" and, on the whole, what satisfies temporarily is satisfying for the individual's entire after-life,—temporary satisfaction being further regulated by the prescience of intellect. On both counts, the individual's gain through behavior involves no counterbalancing loss to the species. For what is good for him, as a total life, is more often than not good for the species; and, in addition, the species itself has power to weight the satisfyingness of affairs. "When behavior arises which, besides satisfying the individual, adds to the sum of satisfaction of the group, it is almost inevitably selected for survival;" care of offspring, for instance, or good will to fellows of the same species, or industry, or inintellectual curiosity and its sequents, scientific investigation and invention, or the sense of justice. "Each succeeding generation's happiness is protected"—not perfectly, rather in a restricted, hand-to-mouth way; but still protected — by these modes of behavior; "and the future generation is always there to protect itself." This meliorism is intelligible; if restricted, it is a doctrine of surety; it is independent of any theory of the interrelation of mind and body; it

applies to thought as well as to conduct; it is matter of observed scientific fact. "To assert that, so far as man's own behavior goes he betters himself, is the same variety of judgment as to say that so far as the behavior of the population of Russia goes it increases itself."

The latter portion of the essay appears to us to contain little more than the generalizations natural to a writer of optimistic temperament. The nerve of Professor Thorndike's contribution lies in his attempt to substitute a workable physiological mechanism, an interplay of cause and effect, for the facile teleology of Spencer and his followers. That the mechanism is hypothetical is no objection; so far as our knowledge goes the nervous system may very well, as the author says, "be so constituted as to produce increasingly those neural arrangements which possess satisfyingness;" and no one can do more than guess at what really happens within it. The present guess was distinctly worth making, and might profitably be worked out in greater detail,— as, perhaps, it will be if Professor Thorndike's promised critique of Jennings evokes a reply. The doubt that remains is the doubt that lies behind his whole "interpretation of human and animal behavior;" the doubt of meliorism itself.

It is with regret that we note the omission of Professor Hyslop's name from the list of contributors to the volume. While, on formal grounds, Professor Hyslop would naturally be excluded from participation, his recent connection with Columbia University, and his sustained interest in matters of which Professor James is also a devoted student, suggest that a paper from his pen would have been especially welcome to the eminent psychologist and philosopher to whom the essays are dedicated. P. E. WINTER.

Die biologische Theorie der Lust und Unlust, von DR. D. C. NADEJDE. Heft I. Leipzig, W. Engelmann, 1908. pp. viii, 99.

This work represents, so far as the reviewer knows, the first serious attempt to grapple critically with the teleological—or, as the writer prefers to term it, the biological—theory of pleasure-pain. Brief critical discussions may, it is true, be found in Wundt's *Physiologische Psychologie*, in Külpe's *Grundriss*, and in Ribot's *Psychologie des sentiments;* still briefer in Ziegler's *Das Gefühl* and in Stumpf's recent paper on *Gefühlsempfindungen;* and a careful search would, no doubt, add to the list. On the whole, however, the adherents of the theory, from Aristotle down to Spencer and Lehmann, have had things very much their own way. Physiological psychology has been content to remark that the rule of correlation, of pleasure with the useful and pain with the harmful, has salient exceptions, or that science may not lay too great stress upon a teleological maxim; it has made its formal reserves, and has then allowed the correlation to stand. A typical treatment of this sort is given, for example, in Ebbinghaus's *Grundzüge*. Such an attitude is no longer possible; all future students of the psychology of feeling must take account of Dr. Nadejde's work.

The work itself is, unfortunately, neither easy to read nor easy to review. The writer confesses, in the preface, to a certain difficulty in the use of German, and this difficulty may account for the somewhat cramped style, and the tendency to string together paragraphs whose logical connection is not always clear. Every one knows that the free and correct use of connectives is one of the supreme tests of a mastery of the German language; but we do not always realize how obscure a piece of otherwise straightforward reasoning may become if the connectives are omitted or their finer shades neglected. And the reviewer meets these further difficulties: that the critical portion of the essay

consists of detailed and highly articulated argument, while the constructive portion is couched in terms of Lipps's psychology, a psychology with a language and an attitude of its own. The author might have materially aided the reader by appending brief summaries to his several chapters; it is possible, however, that a general summary is to be included in the forthcoming Heft 2.

Dr. Nadejde begins by formulating the current teleological theory in two propositions: (1) that there is a connection between utility, or a normal mode and manifestation of life, and pleasure; and between harmfulness, or an abnormal mode and manifestation of life, and unpleasantness; and (2) that there is also a connection between pleasure and furtherance of life, and unpleasantness and hindrance of life. These two propositions, often confused, must for methodological reasons be sharply separated; the former concerns the maintenance, the latter the further advance or development of life. The present Heft 1 discusses the first correlation only. Its aim is threefold: to give a clear and comprehensive exposition of the theory itself; to furnish a detailed critique, from a strictly psychological point of view; and, finally, to reconstruct the theory in unexceptionable form.

We begin, then, with exposition, which naturally starts out from and closely follows Herbert Spencer, although other psychologists are duly referred to. Two points are of especial importance in this chapter. (1) The author brings clearly to light the twofold character of the pleasure-pain correlation. On the one hand, the feelings are regarded as symptomatic; they are signs, indications of a normal or abnormal life-mode. On the other hand, however, they have an active function; they are incentives to and deterrents from action, they guide the vital functions, they stimulate activity for ends. As symptomatic, they refer back to their cause (bodily function); as active, they refer forwards to their consequence (mental and bodily activity); there is interaction between environed organism and pleasure-pain. (2) The arguments offered in support of the theory are of two kinds: inductive and deductive. Greater emphasis is usually laid (as by Spencer) upon the deductive arguments. And these are again twofold; direct and indirect. The direct argument asserts that the biological theory is "an inevitable deduction from the hypothesis of Evolution" (Spencer); the indirect argument seeks to prove the theory by proving the inconceivability of its opposite.

The author's criticism begins with a refutation of this second, indirect proof. The refutation is flat and final. The indirect argument is guilty of two logical errors; it presupposes the very thing that is to be demonstrated, namely, the identity of utility and pleasure, harmfulness and pain; and it mistakes the meaning and nature of the law of causation. You cannot alter cause without thereby altering effect; and the existence of an "objektive Bedingung" necessarily implies the non-existence of its opposite. "It is nonsense to try to demonstrate the impossibility of the opposite of an objective condition by showing its incompatibility with other conditions; for the impossibility of the opposite is already given in its direct contradiction of actual fact, *i. e.*, of the objective condition whose opposite it is." Thus far, then, the ground is cleared.

We pass, next, to a critique of the concept of utility. This is, evidently, a concept of relation,—a *Beziehungsbegriff*, not an *Anschauungsbegriff*. So the question arises: What is the connection or relation upon which utility is dependent, or by which it is conditioned? Or, more briefly: What is the cause of the useful? When the biological theory answers this question directly, it declares: the useful is that which satisfies the needs of life, conditions the equilibrium of the

organism, etc. But such a reply is logically faulty; it explains the concepts of adaptation, normal functioning, etc., but not the concept of utility. When, on the other hand, concrete instances of the useful are given, they always take the form of certain physical conditions of life: oxygen, water, heat, nutrition, etc. And here there is a double confusion. First, the useful is identified with one set of the conditions of our physical life, and with one set only, namely, the external, environmental factors; the influence of the inner conditions, of organized substance, is left out of account. And what holds of the useful holds here also, of course, of pleasure-pain. Secondly, however, since the theory has overlooked the duality of life-conditions and has identified the useful with these conditions at large, the causal connection becomes for it not the connection of utility and pleasure, but that of physical life-conditions and pleasure. All that we have to do, then, is to strip the concept of its borrowed robes, to remember that it is the concept not of 'cause' but of 'utility,' and we see that it represents simply a subjectively conditioned judgment, of the sort with which science can have nothing to do. The concept of utility must be eliminated.

The two following chapters are occupied with the factual exceptions to the rule of correlation, and with the various attempts made to defend the theory in spite of these contradictory instances. The proposed limitations of the teleological interpretation—local, qualitative, temporal—are shown to be of no avail; and Spencer's recourse to an extension of the theory, in terms of an ethics of optimism, brings him into contradiction with his own evolutionist principles. Once more, therefore, the theory fails to meet the demands of logic. What is left of it?

We must attempt a reconstruction, bearing in mind the two cardinal errors of the biologists: the introduction of the concept of utility, with the view of discovering in the theory of feeling a basis for preconceived ethical views; and the confusion or identification of physical conditions with mental phenomena. We may say, in psychological terms, that pleasure is the expression of the direct fulfillment of the conditions, tendencies or needs of the psyche; and that unpleasantness is the expression of an imperilled or indirect fulfillment of these same conditions, etc. The symptomatic significance of the feelings is thus retained. Their active significance must be given up. For the conception of pleasure as incentive or stimulus involves the confusion, first, of feeling with active endeavor (wish, inclination), and secondly of active endeavor with movement, activity, will, *i. e.*, with the actualization of endeavor for the attainment of an end. The uniform connection between pleasure and endeavor rather finds its explanation in the law that all psychical occurrence follows the line of the easier, less resistant, and therefore free and direct realization of its natural tendencies. Pleasure is thus, in other words, an indication that the natural intrinsic tendency of mental occurrence may continue its course unimpeded by the tendency of the object which confronts consciousness; unpleasantness is an indication that the tendency of the external object impedes, inhibits, checks, obstructs this intrinsic natural tendency. Pleasure and pain are neither stimulus nor end, but symptom and symptom only.

These general remarks must now be supplemented by special considerations. First, what of the utility of pain? On the biological theory, pain must be useful, or it could not have 'survived.' Now it is not as symptom of organic damage, but rather as incentive to the avoiding reaction, that pain can be of service. And as we have seen that the active rôle attributed to the feelings depends upon a confusion

of thought, we might dismiss our question without further answer. If, however, we seek for a positive rebuttal, we find facts enough at our disposal. Unpleasant experience may be avoided not only by action but also by inaction; and the 'avoiding' reaction itself may be of two kinds, withdrawal or attack and conquest; and, yet again, unpleasant experience may be overcome, and thus in effect avoided, by habit. Moreover, the unpleasant may have to be sought, as means to a remoter end. And finally there is case upon case of inevitable pain (incurable disease), of pain whose avoidance is impossible. The biological theory breaks down.

What, secondly, of the pathological character of unpleasurable feeling? Is such feeling always a symptom of organic lack or damage? The author argues the question to a negative result in what is, to the reviewer's mind, one of the best reasoned chapters of his study. He takes up, in order, the topics of bodily discomfort (here he befogs the issue, to some extent, by a confusion of pain as feeling with pain as sensation) and of emotional or 'moral' pains and disagreeablenesses. Under the latter heading he points out, *e. g.*, that in the course of an hour's reading one may have had an almost unbroken succession of disagreements, aversions, irritations; vital disturbance, cerebral damage must, then, occur every minute or two! Moreover, it cannot be affirmed, on the other side, that pleasure means always an enhancement of life; there are pleasures from chronic indulgence, pleasures of insanity, the euphoria of the death-bed. It must, therefore, be concluded that pleasure and pain are both alike normal phenomena, and that both swing within wide limits of intensive difference.

With these special considerations out of the way, we can proceed to a final formulation. Pleasantness and unpleasantness are qualitative opposites, normal and positive experiences. Pleasantness is the symptom or index of the fact that a mental process is suited for mental assimilation; unpleasantness, on the other hand, signifies that a mental process is unsuited for mental assimilation. This conclusion is wrought out in terms of Lipps's two "fundamental conditions of the psychical life," *Vereinheitlichung* and *Sonderung*. ——

It is unfortunately inevitable that an ordered presentation of any psychological doctrine must, as things are, be couched in the language of some particular psychological system. The present writer is unable, on many fundamental points, to agree with Lipps. Hence it is but natural that the constructive part of the essay whose contents have been here outlined should seem to him less successful than its critical sections. It must, however, be said that a translation of the author's position into other terms is easily possible. As criticism, the essay is very certainly deserving of high praise. Dr. Nadejde has attacked an obvious and important problem which has been neglected, or at least but very partially treated, by his contemporaries. He has thus done a real service to scientific psychology. Such problems are, of course, not to be solved out of hand, and there will, without doubt, be reply and counter-reply and reply again. But, at any rate, a shrewd blow has been struck at the teleological interpretation of the feelings.

E. DANBY.

Philosophie der Werte: Grundzuge einer Weltanschauung, von HUGO MÜNSTERBERG. Leipzig, J. A. Barth, 1908. pp. viii, 486.

It was in 1808, exactly a century ago, that Johann Gottlieb Fichte issued his "Reden an die deutsche Nation." The tremendous influence of this work is well known. As Professor Münsterberg puts it, Fichte's "Weltanschauung war ein künstliches Denkgebilde, aber wieder bewährte es sich, dass der Idealismus des abstrakten Denkers

im Grunde die lebendigste und wirkungsvollste Lebensmacht ist. Sein reiner Glaube an die ewigen Werte ergriff die tiefste Seele des Volkes." But Fichte's idealism presently sank into disrepute, overwhelmed by the positivism of natural science. There was only one world, the world of observed fact; and all the 'riddles of the universe' were set by that and must be answered, if they could be answered at all, in terms of science itself, of elements and laws of occurrence. Then came the inevitable reaction against positivism, and the steady trend towards epistemology and away from metaphysics, that are characteristic of current philosophical thinking. Facts in themselves become tiresome; the thinker turns "zur Erkenntnisfrage, zur Frage nach dem Wert der wissenschaftlichen Behauptung;" he begins to enquire "nach Sinn und Bedeutung." So the concept of *value* is brought to the forefront of philosophical interest. A mere "Umwertung der Werte" is, however, not enough: we must, says Professor Münsterberg, push our investigation to "das tiefste Wesen der Bewertung." "Die Gesammtheit der Werte muss grundsätzlich geprüft und aus einer Grundtat einheitlich abgeleitet werden." What our latter-day philosophy lacks is "ein in sich geschlossenes System der reinen Werte;" only when we have this "kann die Philosophie auch wieder aufs neue zur wirklichen Lebensmacht werden, wie es zu lange ausschliesslich die Naturwissenschaft gewesen ist." Its provision is by no means easy, but rather a matter of keen and laborious thinking; it will not suffice "die grossen Gedanken des deutschen Idealismus noch einmal auszusprechen." Natural science has intervened; more especially "die Naturwissenschaft vom Seelischen, die Psychologie, hat ganz neue Ausblicke eröffnet; neue Wertgebiete des praktischen Lebens haben sich aufgetan; wir sind andere Menschen geworden." The day for reconstruction may, perhaps, not yet have arrived; but its arrival may be hastened.

In this spirit, the author lays before the public his own Philosophy of Values, which falls into two parts, a brief introductory theory of values and a system of values. The book is designed to turn our attention from laws to ideals, from the pleasurable and the useful to the sphere of pure duty, from material things to free will, from the world of facts to the world of eternal values. That it is a notable attempt goes without saying; it is the expression of a strongly marked personality, and shows a sustained fervor of conviction. Nevertheless, it is, in the fullest sense of the phrase, a "künstliches Denkgebilde;" and without running to the other extreme, of a raggedly pragmatic universe, we may surely say that the age of such systems is past. Individuals, men of like temperament with the author, will find in his pages inspiration and encouragement. All readers of the book will derive from it the benefit of a moral tonic. The circle of Professor Münsterberg's admirers will be increased. But that the doctrines which he sets forth will appeal, vitally and enduringly, to any large body of academic youth, whether in Germany or in the English-speaking world, seems hardly possible. The next reformation in thought must come from within the sciences, not from the external realm of concepts. M. W. WISEMAN.

The Philosophy of Loyalty, by JOSIAH ROYCE. The Macmillan Co., New York, 1908. pp. xiii+409.

These lectures were first delivered in their present form before the Lowell Institute in the autumn of 1907, although their substance had been given in various places at other times. "It is simply an appeal to any reader who may be fond of ideals and who may also be willing to review his own ideals in a somewhat new light and in a philo-

sophical spirit," and its aim is to simplify the moral issues of the day, in which there is restlessness regarding the foundations of morality.

Loyalty is defined as "the willing and practical and thoroughgoing devotion of a person to a cause." It is never merely emotional, it involves self-control, and it has a social cause. It has value for the individual, because it frees him from doubts and unifies his life. By nature we have no personal will; we find our will in part by imitation, social conformity, and when this proves insufficient, we swing back to caprice, finding satisfaction in neither, until we weld both factors in loyalty to a cause, "So rich, so well knit and to him so fascinating and withal so kindly in its appeal to his natural self-will that he says to his cause, 'Thy will is mine and mine is thine. In thee I do not lose but find myself, living intensely in proportion as I live for thee.'"

The philosophy of loyalty agrees with individualism in recognizing that no impersonal theory can be successful, that each person is the centre of his own world and must have his own end. But it points out that the ends set by mere individualism do not solve the individual's problems. Mere self-assertion in any form, thinks Prof. Royce, is an empty and futile process, because there is no self until it is made concrete in social relations. On the other hand, mere subjection to social relations also destroys the self, so that our only way of realizing the self is to combine our individuality with society in the form of loyalty to a cause.

Such a cause must be one in which the person can realize himself fittingly, and this can happen only if it joins many persons into the unity of a single life. It must be both personal and super-personal. Loyalty to my cause must also help to sustain others in loyalty to theirs, and in so far as my cause prevents this, it is a bad cause. Loyalty to loyalty is the final end and standard in selecting our cause, for this is simply finding the harmony between self and the world in some practical work. Such a standard gives conscience a touchstone to decide doubtful cases by because it furnishes an ideal and thus virtually creates a self. In a conscientious person doubts arise when loyalties conflict with each other, and in such a case our principle of loyalty to loyalty says, "Choose that which is more loyal to loyalty. If you do not know which is the better cause, at least decide, even if you must do so ignorantly, and having chosen, abide by your choice with courage and loyalty. Even this is better than hesitancy."

Loyalty is the great need in America to-day, first to the family, and secondly to political institutions. The latter gives rise to an especially difficult problem because our country is so impersonal and big that it is hard for men to be loyal to it as of old. They are loyal, intensely so, to their class, but not to their country. We need a new provincialism, a loyalty to city, and to state, as a training for the wider loyalty to our country.

Training for loyalty begins in early childhood with the idealization of heroes; it is found to some degree in gangs, codes of honor and the like; at adolescence it crops out especially in fraternities and loyalty to one's own side or institution. These natural forms should be utilized and guided by wise leaders, towards an idealized cause which demands strain and sacrifice from its followers. Idealization of the cause is of all these the most important, as is seen in the fact that some of the "lost causes" have been those which have most influenced the world, such as the idealization and transformation of the Kingdom of Israel into the triumph of righteousness. Through the power of grief and imagination loyalty to such a cause is so heightened that the

man is stimulated to ever greater efforts and to a union of his cause with that of all loyal souls, with art and with religion.

Loyalty thus comes to appear as the service of a superpersonal cause, and involves a higher unity of consciousness than that of ordinary human individuality. That is, "the social will is a concrete entity, just as real as we are and of a still higher grade of reality than ourselves."

At this point Prof. Royce makes connection with his theory of the Absolute. The faith of the loyal has truth; a world of truth implies a world possessing a rational and spiritual unity, a conscious world of experience, whose type of consciousness is higher in its level than is the type of our human minds, but whose life is such that our life belongs as part to this living whole." He then proceeds to a criticism of Prof. James's statement of pragmatism.

James says, "'The true,' to put it very briefly, is only the expedient in our way of thinking, just as 'the right' is only the expedient in our way of behaving." "It pays for our ideas to be validated, verified. Our obligation to seek truth is part of our general obligation to do what pays. The payment true ideas bring is the sole why of our duty to follow them."

First of all Royce agrees that the search for truth is a practical activity with an ethical purpose, and that its attainment means a practical success in active undertakings. But what constitutes success? Surely not the success of the mere instant, not the expediency which views only to-day. James himself inserts the phrase "in the long run" to broaden his expediency and success. But when does a man experience all the facts about the long run? "To appeal to the genuinely real 'long run' is only to appeal in still another form to a certain ideally fair conspectus of my own whole life, a conspectus which I, in my private human experience never get." That is, the admission of the long run, means that we judge our success by some ideal,—which Royce seems to assume to be an Absolute—instead of by the instant.

That is, says Royce, upon the basis of our need for unity, we form ideas of what such a unity of experience is. If these ideas are true, "then such a unity does actually exist and is experienced in some conspectus of life which wins what we need, approves our loyalty, fulfills our rational will, and has in its wholeness what we seek. And then we ourselves with all our ideas and strivings are in and of this higher unity of life." This is an eternal truth. To deny this reality only reaffirms it in another form, saying that the whole truth is that there is no truth, and hence we derive the proof that truth and a real world actually exist.

Now this truth is simply the Eternal, conscious, united, self-possessed, and perfected through our devotion to it. Loyalty thus becomes "the will to believe in something eternal and to express that belief in the practical life of a human being," and success and concord with the truth come in so far as each person is in unity with this Eternal or world consciousness.

Prof. Royce's criticism that when Prof. James appeals to the long run he virtually appeals to an ideal conspectus, is indeed a valid one, and if the long run is carried out to its logical end, pragmatism must admit that the final practical testing of any act is never finished until the universe itself comes to an end. That is, pragmatism makes its appeal to faith as a principle of action, saying, "Be bold! Absolute truth is unattainable, and if man is to live at all, if he is to act at all, he can do so only by a prophecy of success and the effective faith in his prophecy that makes him act as if the prophecy were already a

fact." If the results are harmonious with the rest of his and other men's experiences, then *in so far* he has a guarantee of fact and truth, although theoretically the widening experience of mankind may put this particular act out of commission at some future time. The reviewer has not yet been able to see why any pragmatist may not have a completely worked out *theory* of an ideal world, perfection, and so on, which serves as his working hypothesis, but of which the *truth* is tested by every act, and which must be modified accordingly.

This, however, is far from saying that such an ideal or that any ideal is a *fact* outside of individual experiences or that there is an *actually existent* objective unity of experiences, of which each person's individual life is only a fragment. It is difficult to see upon what grounds Prof. Royce makes such an assumption. The person who denies such a reality does not need to affirm that the whole truth is that there is no truth. He may say that assertions about "the whole truth" can refer to facts only if they refer to the present totality of experiences—including, of course, all ideas and ideals, and that such "whole truth" develops with the development of life and mind. To assert that at each instant there must be somewhere a conscious unity of all experiences is surely an assumption which rests upon the pragmatic motive of the individual's need for it in order to unify his own thinking. As such it may be altogether justifiable, but it should be held as an assumption and not as a demonstrated fact.

Considering the book as a whole, it will doubtless appeal to a large class of readers who stand in just such need as the author mentions of some stimulus to deepen and make more serious their moral lives. There is a great deal of repetition, not only of phrases but even of paragraphs and almost of pages, but this may be of value in fixing the few fundamental ideas which the author is trying to enforce.

AMY E. TANNER.

Etudes d'Histoire et de Psychologie du Mysticisme, Les grands mystiques chrétiens par HENRI DELACROIX. Paris, F. Alcan, 1908. pp. 470.

For this psychological study M. Delacroix selects Saint Theresa as a representative of the Spanish mystics of the sixteenth century, Madame Guyon, of the French mystics of the seventeenth, and Suso, of the German mystics of the fourteenth. He selects these three especially because they have left letters and autobiographical writings freer from doctrinal taint and with more pure introspection than have some other and perhaps better known mystics.

The mystic is a person who believes that he apprehends immediately and internally the divine presence. His tendency is complementary to if not opposed to rationalism. It is usually based upon or in ascetism and self-renunciation in various forms, and is marked by passivity, a sense of the divine presence, and loss or enfeeblement of the feeling of self and its mental functions. In its most typical forms it passes through three stages: a stage of exaltation, of various degrees, from contemplation up to complete trance, which is only temporary and which holds the mystic passive; second, one of sorrow or pain, intensified by the contrast with the previous state and corresponding somewhat to the modern "conviction of sin;" and third, a permanent consciousness of divinity, which is not characterized by trance visions, or any of the other somewhat abnormal marks of the first state, and which heightens the mystic's bodily health and powers and makes him able to deal with all sorts of practical problems to a greater degree than before. The first stage is not uncommon, the author believes, and is found often in persons of marked hysterical tendencies. It is often brought on or heightened by fasting and bodily

mortification and is often marked by hallucinations of various sorts as well as by catalepsy and sometimes epileptic attacks. This first stage, with these phenomena, is, by the great mystics, looked upon only as preliminary and as untrustworthy in itself. They say that their visions cannot be trusted, except as they lead to greater efficiency, for visions may be from the devil as well as from God; which, to modern psychology, means that nervous tendencies with a vivid imagination may lead either to a poem, an invention or to crime. But in the third stage, in which the presence of God is constantly felt, the mystic cannot doubt the divine presence because he judges it by its works, its effects upon all his daily activity. He feels within himself a power directing every thought and movement, doing tasks beyond his previous powers, giving wider scope and force to his mind, depth to his affections, and energy to his will. It acts like an external force, and he never questions that it is external and divine.

Why does the mystic have such an experience? Various factors unite, in the opinion of M. Delacroix. At the basis of it lies an unusually rich subconscious self, and along with it, a tendency to lose the conscious self in intuition. These are essential for the internal experiences. In addition must be included the somewhat abnormal life led by the great mystics, usually celibate, with fasting, perhaps scourging, and other forms of bodily privation, in some instances with long attacks of illness, or distinct neurotic tendencies. All these make vision and trance easy, and alone would tend to the passive forms of mysticism so common among Orientals. But Christian mysticism in its highest forms advances to the third stage because these mystics have been brought up within the church and have in their subconscious selves a vast fund of Christian tradition towards a life of practical piety. They have usually been faithful children of the church, under the care of a director, or under the influence of church writings, and all the suggestions from these sources have reacted beneficially upon the natural tendencies, to an outcome in which we find a union of contemplation and action which in its value and perfection can best be compared with the highest forms of art. The mystic's life is his work of art, shaped as the artist shapes his statue.

In giving this psychological explanation of mysticism the author by no means intends to discredit it. He rather aims to give a new form of justification to the tendency, emphasizing throughout its beneficial effects upon the person. He studies in great detail the experiences of Saint Theresa, Madame Guyon and Suso, even to the point of over-much repetition when he comes to the summing up; and the history of the controversy between Madam Guyon and the church, with the quarrel of Bossuet and Fenelon, seems aside from the aim of the book. On the whole, however, the book is both an interesting and valuable addition to the psychology of religion.

AMY E. TANNER.

The Inward Light, H. FIELDING HALL. The Macmillan Co., N. Y., 1908. pp. 228.

This book is a presentation, largely through symbols and figures of speech, of the teachings of Buddha as found among the Burmese today. The writer says that western writers have failed to understand Buddhism: 1, Because they have assumed that the formal teachings of Buddha are the whole of Buddhism, whereas in addition there really is all the teaching that preceded him and which he only complemented; and 2, they assume that the fundamental conceptions of the universe are the same for the West as for the East, which is not so. Before we can understand what Buddha taught, we must, then, know the underlying conceptions of life of the East, and the social

system upon which he built his teachings. A Westerner fell sick in a Burmese village and was cared for by the monks there. As health came back, for the first time he saw the beauties of nature and the goodness of life, and he asked himself where his life had brought him so far. Whither do life and work lead? What is life? He began to ask the monks and the villagers. He found that they conceived of Life as one great whole, of which all lives are but different manifestations, and this great Life has a Consciousness, a Righteousness and a Knowledge. We call it God, but this word is mixed with ideas of personality and limitation quite foreign to Buddhist thought. The world is the living garment of this Life, and all the forms in it have evolved from lower forms, all obeying their laws, all happy, even in death. The soul, too, of man, has evolved, not bearing a conscious memory, but in its present height and development showing its past righteousness or sin, acquiring 'merit,' ever acquiring greater power, ever widening, until it reaches a perfection that makes it existent without the limitations of matter. Each personality is a beam of the eternal sun, and each is necessary to make the ray of pure white light,—the rays below the spectrum, which make up unconscious life, all those in the spectrum, and those above it—all are needed for the white ray. Love gives the immortality and power of the unconscious life, without which the Soul could not exist, and in so far it is divine. The immortality given by love widens into that of the community, and that into the nation, but always below the nation is the family, and if its truth is not kept, then will the nation die.

There came a famine and many died because, says the West, they were ignorant and thriftless. But the East answers, "you live ever in fear of famine. We would rather suffer famine than fear, for famine is only of to-day, but fear is a part of the soul." The East has ignored the body, in its submission to fate, but the West has ignored the soul in its desire for freedom, and has become the deeper fatalist of the two. All ills pass. Justice underlies all, in this life or another. Be therefore of good cheer. Laugh, for death is but the beginning of a new life. New things are ever ahead for him who follows the Way, and merges the lesser into the greater Life by self-renunciation and self-denial. He who follows this Way finds peace. Each must keep his own truth, that it be not forgotten, but each must also remember that others have their truths to keep, which, too, must not perish from off the earth. Men have their truth to keep, a truth more primitive and stern than women's; and women have their truth, which is best expressed in the truth of Buddhism and of Christianity, and which rests upon the truth of men as the pagoda spire rests upon its foundation.

God, then, is the pure white ray (not a personality, a limited God), but the greatest realization of the Infinite God. To it we cannot pray as to one who may be moved by prayer, but is this the root of prayer? Not so. Our need of prayer comes from our isolation, ignorance, lovelessness. We want the inward light that never dies, for only so can we never be cast aside and forgotten. This is what prayer does—brings our tiny ray of light into the infinite ray, completing us and it in a perfect fellowship. Hell, then, is but the present suffering our present sin entails, and Heaven the everlasting reward of growth to the struggling soul, the greater light, the becoming one with the Infinite Ray. That is Nirvana.

Throughout, the book is a very simple, beautiful, sympathetic presentation of a great religion. It brings out similarities between the Eastern and Western modes of thought that cannot but increase our consciousness of the fundamental unity of all religion.

AMY E. TANNER.

Geschichte der Autobiographie. Von G. MISCH. Bd. I, Das Altertum. Leipzig und Berlin, B. G. TEUBNER, 1907. pp. viii, 472.

In 1900 the Prussian Academy of Sciences made the History of Autobiography the subject of a prize essay. The present work—which is to be completed in three volumes—was submitted in competition for the prize. Whether the author was successful we are not told; had he been, we should probably have read 'preisgekrönt' on the title-page. However that may be, the book is interesting and important; and if there is to be another publication under the same title, we may assume that the two histories will no more duplicate each other than in the analogous case of Dessoir and Sommer.

This Vol. I treats of autobiography in the ancient world; in the Hellenic and Attic, and in the Hellenistic and Græco-Roman periods; and lastly in that typical age of self-examination which culminated with St. Augustine. Vol. II, which is to appear shortly, will bring us down to the seventeenth century, and Vol. III will complete the history to our own times.

In his introduction, the author points out the relations of autobiography to history, psychology and sociology. We have in it self-portraiture and self-criticism, clothed in the language of the 'inner experience;' we have in it a reflection of historical fact, of actual occurrence, as the varying and various interests of the writer lead him to record the objective experiences of himself and of his fellows; and we have in it a description of social and political relations, of the "Selbstbehauptung des politischen Willens," of the relation of the author to his work and to his public. The primary motive is that of self-portrayal, of *Selbstbesinnung*. This, however, may wear many different forms, appearing as reflection upon character and human relationships, upon the conduct of life, upon human destiny, upon religious conceptions. Only gradually does it take on its modern significance. "Der eigenste Kern der europäischen Selbstbesinnung ist die Gestaltung des Lebens aus dem Bewusstsein der Persönlichkeit. Dieses Bewusstsein gehört nicht zum gemeinsamen Besitz der Völker, im hellen Licht steigernder Kulturarbeit ist es allmählich erworben worden, es kennt die mannigfaltigsten Weisen und Stufen, und auch die anderen Richtungen, die Lebensweisheit und die Erhebung zum Ewigen, gewinnen erst durch die Verbindung mit dem Persönlichkeitsbewusstsein ihre diesseitige Vollendung—in solcher Selbstbesinnung wirkt die Autobiographie mit an der Befreiung und Vertiefung des Lebens." This developmental view of self-examination, which "den innersten, langhin erst gleichsam unterirdisch verlaufenden Zusammenhang der Selbstbiographieen herstellt," is the key-note of the work.

The author begins with the autobiographical narratives of the elder oriental nations, of the Egyptian and Babylonian-Assyrian epochs. If we are to measure productivity by number, the autobiographical work of this age hardly lags behind that of the next two thousand years, from the beginning of the classical period to the end of the Renaissance. But "in dieser Fülle ist eine unendliche Armut an Individuellem:" autobiography is in its collective, impersonal stage; the motive of self-examination is working underground; self-consciousness has not yet arisen. When we come to the Greek period, we are on more familiar ground, on the ground where the continuity of history begins. Here, however, "tritt zunächst ein merkwürdiges Missverhältnis hervor: in dieser griechischen Kultur, die den Menschen entdeckt und befreit hat und so viele Formen, ihn darzustellen, fand, hat die Autobiographie nur einen beschränkten Raum, sie erscheint als eine literarische Spezialität von sekundärer Art." The author

goes carefully and seriously to work to account for this anomaly, and shows that, in spite of all appearances, the thread of *Selbstbesinnung* is never broken. In the Hellenistic period ("es ist ein Ruhmestitel der Forschung unserer Tage, dass sie die weltgeschichtliche Bedeutung der hellenistischen Epoche zur Anerkennung brachte") autobiography comes to its rights: die autobiographische Gattung hat im Hellenismus zuerst ihre zusammengesetzte Gestalt gezeigt, und in diesem vielköpfigen Wesen erhielt Persönlichkeitsbildung endlich das Uebergewicht. Dadurch häugtdie Autobiographie nunmehr engermit dem Prozess ihrer Zeit zusammen, mit deren Grösse und mit ihren Schranken." But the fourth and fifth centuries, which saw the decline and fall of the antique life, the life of the ancient world, were the Augustan age of autobiography. "Die Autobiographie gewinnt eine Form, mit der sie sich ebenbürtig zu den Gattungen der grossen Kunst gesellen kann." This, indeed, is the one artistic triumph of the time. "So dicht erfüllt mit Kultur die überreife Zeit war, sie hat doch aus dem bunten Leben keine eigenen Kunstformen zu gestalten vermocht, das Christentum . . . hat überhaut bei seiner Ausbreitung in der alten Welt nicht die Kraft bewiesen, neue Literaturgattungen zu erzeugen, und selbst Augustin hat es nicht vermocht, sein Tiefstes objektiv, losgelöst vom persönlichen Erleben voll herauszubringen. Nur die Selbstbiographie hat eine neue Blüte getrieben."

Such, in outline, are the plan and teaching of this first volume. It need not be said that the author brings a world of interesting detail; the chapters on Cicero, on Seneca and Epictetus and Marcus Aurelius, on Gregory of Nazianzus, on St. Augustine, would be interesting in themselves and apart from their context; they are doubly interesting in the context and connection that the author gives them. A technical review of the book would be the task of specialists in many fields. As a whole, it strikes the reviewer as a solid and notable piece of work. F. E. BARBOUR.

Psychology, General Introduction, by CHARLES HUBBARD JUDD, Ph. D. New York, Scribner's, 1907. pp. xii+389.
Laboratory Manual of Psychology, pp. x+127.
Laboratory Equipment for Psychological Experiments, pp. vii.+257.

In the words of the author, as expressed in the preface, "This book aims to develop a functional view of mental life. . . In the second place, I have aimed to adopt the genetic method of treatment. . . In the third place, I have aimed to give the physiological conditions of mental life a more conspicuous place than has been given by recent writers of general text-books on psychology. . . In the fourth place, I have aimed to make as clear as possible the significance of ideation as a unique and final stage of evolution."

This is an excellent programme though it contains little that is new, as, indeed, it should not. A text-book in any science should have for its main purpose a clear and systematic presentation of the known facts in the science. Novel theories and individual points of view belong in special treatises.

The subject is throughout the book approached from the logical instead of the psychological or pedagogical point of view. This is not always fortunate from the teaching standpoint.

After a general introduction, to which Chapter IV on the general analysis of consciousness belongs, the author begins with the evolution of the nervous system, and then passes in successive chapters to the human nervous system and to sensations. The traditional five senses are discussed and the end-organs are illustrated, but the kinesthetic and organic sensations are scarcely mentioned.

The psychological aspect of sensations is treated in Chapter VI. This chapter really deals with perception, but this is not at once evident. The author seems to assume that the student already knows what perception is. After some general remarks that are not altogether lucid, the student is immediately launched into space perception. This is not approached from the side most familiar to the student, from space perception as simultaneously contributed to by the various senses, but is approached from the analytical side. Tactual space, auditory space and visual space are taken up in succession. It is here that the weakness of the logical, as compared with the psychological, method comes out more strongly than anywhere else in the book. A class is at a loss to know what it is all about.

The heart, and really the natural starting point, of perception is next treated in a section entitled "Unity of Objects." This section stands out as relatively clear and to the point, but it does not adequately cover the subject. The section on 'Time,' which comes next, is disappointing in its vagueness. The author makes no attempt to show what the experiences are from which one gets the concepts of 'past,' 'present' and 'future,' but devotes all his space to a discussion of the scope and origin of 'the present.'

A teacher using the book with a class would do well to begin this chapter with the section on the unity of objects, supplementing it liberally, to develop space perception independently before taking up the sections on tactual, auditory and visual space, and to omit the section on time.

The term 'experience' is used throughout the book as synonymous with consciousness, and not in its usual sense of the interaction between consciousness and the outside world. Chapter VII, entitled 'Experience and Expression,' deals with the motor nature of consciousness and with feeling. At the beginning of the chapter it is implied that all movement, even that of the internal organs, always influences consciousness. The uniform connection of consciousness and activity is brought out by numerous illustrations and references to experiments.

Attitude is indiscriminately treated both as an incipient reaction, illustrated by the attitude of attention (p. 189), and as synonymous with feeling (p. 193). This is likely to cause confusion in the minds of the class unless it is previously pointed out by the teacher.

Feeling is described as referring to 'intimate personal states, rather than experiences in which reference can be made to external objects' (p. 193). All mental states having subjective reference are here included. Feeling cold, a sensation, is put into the same class as anger and love, emotions. The terms emotion and sentiment are given passing notice in a later chapter (p. 298), but it is implied that there is little ground for them in psychology. In fact, one has the feeling all through the book that the real emotions and the higher sentiments are explained away. They have no place in Judd's scheme.

The James-Lange theory of the emotions is rejected, but no evidence is submitted on the other side beside the mere statement that "it has been shown that muscular contractions accompanying a feeling take place in time after the consciousness of the feeling has been fully established" (p. 195). "Feeling in its relation to bodily activity always reflects the harmony or lack of harmony of active tendencies" (p. 196). Since the James-Lange theory has been rejected, it must be assumed that the harmony or lack of harmony is only the occasion for arousing feeling, but this seems not always to be strictly adhered to (pp. 200, 204).

No inherent qualitative difference in the feelings as such seems to

be recognized. Are not anger, love, sympathy, æsthetic feeling, etc., distinct characteristics given in the native equipment? One would think not, from much of Judd's discussion in this chapter, especially the discussion on the appreciation of art and harmony (pp. 203-211). Just what is the source of these appreciations is not made clear, but they are not taken to come from sensation. In the chapter on instincts, native endowment for these capacities seems to be granted. This makes the treatment appear chaotic, but the psychology of the feelings must remain vague and chaotic till more is known and accepted regarding the functions of the brain. It will no doubt be found that the capacities for the fundamental feeling qualities have definite localizations in the cortex.

Instinct and habit form the subject of the next chapter. Comparatively little space is given to the instincts and no attempt is made to enumerate them. This is not surprising, for psychologists are not agreed as to what the fundamental instincts are, but until these are known psychology, as a science, can make little progress. They comprise the foundation of the science. We now have little more than opinions as to what the fundamental qualities of mind are, what is instinctively given and what is derived, what are the native capacities and at what point organization due to education and training begins.

Memory and ideas form the basis of the next chapter. This is an unusual combination of subjects and is of doubtful success. On page 241 ideas are described as modified memory images, and number forms are injected (p. 242) as evidence of the subjective character of ideas. But on page 244 f. ideas are described as the product of selective attention or analysis and abstraction, and this without any previous discussion of the nature of selective attention. In fact, selective attention and its implications are treated nowhere in the book, and it is this that makes Judd's treatment of the ideational processes so mystical and vague. It is minus its cornerstone.

The principles of association are briefly discussed in connection with memory. Scarcely a page is given to this subject, which is so fundamental in all mental organization. Two principles are recognized, contiguity and similarity, and no attempt is made to reduce them to one.

In training the memory, natural retentiveness and method of memorizing are not distinguished (p. 234). This leaves one in doubt as to just what the author means when he says that memory can be improved, but he seems to imply that natural retentiveness can. No evidence on the point is submitted.

The chapter on language follows the chapter on memory and ideas, and precedes the one on imagination, conception and reasoning. Since oral language is conditioned by conception and the higher thought processes, its exposition should clearly follow the discussion of these topics. Oral language also presupposes an exposition of selective attention. The chapter, however, ranks above the average in the book. The development of language, both oral and written, its social implications, and the manner in which words convey meaning are brought out.

In the chapter on imagination and the formation of concepts we again have an unusual and arbitrary combination of subjects. Imagination is closely related to memory, and ideas in their usual meaning are closely related to concepts, but these subjects are not so joined by our author.

Imagination is described as the radical change of memory images in the process of recall (p. 275). Individual variations in imagery are treated in connection with memory (p. 232). The personifying tend-

ency of primitive people is treated as the result of imagination rather than the result of uncritical perceptive inference, as it is now being regarded by anthropologists and historians. Imagination no doubt enters here after the process is started, but its beginning rests on a naïve assumption in which imagination has little or no place.

The function of the imagination in science is next set forth. Where empirical tests are inapplicable to imagined theories, their test is said to be "coherency of internal organization" (p. 277). Whether the author means here a mere internal congruity of parts, or that the theory must square with all known facts is not clear, but he seems to mean the former. This is indicated by the way he accounts for the postulation of the ether. "It is demanded in scientific considerations in order to make the idea of the solar system and the universe a coherent thinkable idea" (p. 278). Astronomers and physicists always convey the idea that the postulation of the ether is demanded by the undulatory theory of light,—something must undulate.

The development of a scientific concept is illustrated by the growth of the atomic theory and by the theory of gravitation (pp. 282-4). Such a concept he seems to identify with a scientific theory. It is defined as a scientific construct. Used in this sense, conception is not easily distinguished from imagination, and the distinction the author draws is a relative one only. "The image factor has become too attenuated, and the relational factor too important, to justify giving the construct a name which emphasized the image (p. 285).

In the usual sense of a symbol standing for a general notion or carrying meaning, the term concept is also used (p. 287-8). This phase of the subject is treated at some length in the chapter on memory and ideas, p. 237 f., and again in the chapter on language, p. 266 f., but without applying the term concept or conception to it. Judgment and reasoning follow the treatment of concepts.

To the concept of the self an entire chapter is devoted. The chapter on the whole is probably the most abstract and obscure in the book, owing in part, no doubt, to the nature of the subject. A good point is made, one that is seldom made explicit by psychologists, when the author says that in addition to the attributes of other concepts, that of the self "is characterized, also, by a unique type of activity which we do not know to exist in any of the forms of reality about us, other than personalities like ourselves. This type of activity we describe when we use the word 'consciousness'" (p. 311). Much is made of the unity of the self and its relation to the conceived unity of the external world. The author seems to assume that every one thinks of the world as a unified whole and that this unity is read into it because of the unity of the self. This is doubtful. Many people are distinctly conscious of the time when they gain a unified conception of the world, not having had it before. It usually comes as the result of study and experience, but in some it comes so gradually that it seems always to have been there.[1]

The remaining three chapters,—XIII, Impulse and Voluntary Choice; XIV, Forms of Dissociation; and XV, Applications of Psychology,—are among the best in the book, but lack of space forbids a detailed examination of merits and demerits. The discussion of voluntary choice is left somewhat obscure because the author assumes that the reader is familiar with the free-will controversy, which the beginning student seldom is. The question at issue is nowhere clearly stated, which leaves the student at sea. This omission of a statement of the problem under discussion is a frequent fault of our author and accounts for

[1] *Cf.* W. C. Ruediger: The Period of Mental Reconstruction, *Am. Jour. of Psych.*, July, 1907.

much of the vagueness throughout the book. Pedagogically this is a serious fault and seriously hampers the usefulness of the book as a text.

The laboratory manual of psychology gives a good selection of typical experiments for a laboratory course, and is bound to prove useful, especially to beginning teachers of psychology. The apparatus to be used with these experiments and suggestions for setting it up and for carrying out the experiments are given in the volume on laboratory equipment.

W. C. RUEDIGER.

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The Psychology and Pedagogy of Reading, by EDMUND BURKE HUEY, A. M., Ph. D. The Macmillan Company, New York, 1908. pp. 469.

Dr. Huey's book is well written, largely devoid of technical expressions and particularly well adapted to the needs of the class of readers which it aims to reach.

The book is divided into four principal parts: (I) The psychology of reading (9 chapters). (II) The history of reading and of reading methods (4 chapters). (III) The pedagogy of reading (6 chapters). (IV) The hygiene of reading (2 chapters).

Chapter I, taking the form of an introduction, deals with the mysteries and problems of reading from its beginning in Babylonia, Egypt, and Crete, more than 7,000 years ago. Chapter II treats of the work of the eye in reading. The eye, as is well known, does not move continuously in reading but by a succession of quick, short movements, with one usually unbroken return movement. As to the rate of movement, the results obtained by Dearborn, who used Dodge's method of photographic registration, are accepted. Chapter III investigates the amount of reading matter perceived during a reading pause. This has been measured, with fairly congruent results, by Erdmann and Dodge, Messmer, Cattell, Zeitler and others. It varies with the reading matter. There are limiting factors both on the side of eye structure and function, and on the psychic side. In consequence of the former, the larger the amount read during a reading pause, the more inevitably must be the reading by suggestion and inference from clews of various kinds. The deficient picture of the page is filled in and retouched by the mind; from which it follows that reading must go on by other means than the recognition of letters, and that the number of necessary movements and pauses per line will vary with the nature of the matter read. On the psychic side we are confronted by the narrowness of the attention span and by a third limiting factor, the necessity of interpretation.

The fourth chapter bears the caption "Experimental Studies upon Visual Perception in Reading." Herein are considered the mental processes concerned in perceiving what is presented on the page, and the means by which the mind takes note at such a rapid rate of what is there. The theory of reading by letters is obsolete. Already early experiments by Cattell had led to the conclusion that we read in word-wholes, or sometimes even phrase and sentence wholes. The results of experiments by Erdmann and Dodge argue strongly for the theory of the perception of word-wholes. As these authors point out it is not the constituent parts of any given form that make it recognizable, but the familiar total arrangement. While, of course, it is always possible to analyze the whole into its parts, we do not do this in actual reading any more than in regarding a landscape. Goldscheider and Müller ascertained that there are so-called determining and indifferent letters in words. With increase of familiarity fewer and fewer clews suffice to touch off recognition of a word or phrase. Zeitler's experiments led to the conclusion that the apperception of domina-

ting parts or complexes is the basis of recognition. Word length or total form are minor factors. We arrange, in fast reading, the dominating complexes one after another. The progress may be as rapid as one pleases, but it is none the less successive, though long practice and familiarity with the words may give one the 'illusion' of reading simultaneously what is seen during a reading pause. Messmer's experiments on the whole confirmed these facts. The greater importance of the first half and the upper part of words are additional results of experiment. Chapter V contains a discussion of the nature of the perceptual process of reading. It is difficult to draw final conclusion in the present state of science. All we can do is to survey the collected data and dwell on certain general features.

Chapter VI deals with inner speech during reading and with the characteristics of speech. Though purely visual reading is quite possible theoretically, auditory or motor processes form with most people a constituent part. Language and reading being so intimately connected that we can say with Egger that "to read is, in effect, to translate writing into speech," an examination of the nature of speech generally is resorted to in order to throw some light on the inner speech of reading and its relation to the interpretative processes. This examination, which draws freely on Preyer, Wundt, Joubert, Egger, James and Scripture, brings forth the followiug important facts: Language begins with the sentence, sometimes but a single word, yet standing for a total idea. We begin with a total meaning and a total intention of expressing this meaning, the development being toward a more and more particular division of it into aspects or parts. Meaning leads, and the idea of the whole dominates the parts. The total idea is not a mere sum of associations, but is an apperceptive unity. Again, if meaning welds the parts of a sentence into unity, the necessities of physical utterance contribute to the same end. The complex machinery required for vocal utterance is in continuous action throughout the utterance of any word or phrase, with no interruptions such as letters, syllables, or even words suggest. Chapter VII discusses the functioning of inner speech in the perception of what is read. The inner readiness for a new combination of words completes itself so readily from a few visual clews that it is unitarily perceived quite as truly as if it existed as a specific memory-whole. But the habits of inter-association and expectancy which bind the units of language into wholes are most deeply founded in the audito-motor mechanism of speech. Even in silent thinking the organization of our speech habits goes on perfecting itself. The carrying range or span of the inner speech being considerably larger than that of vision, the visual range is itself enlarged and its content supported by the more stably organized inner utterance into which the visual percepts are constantly being translated. The reading matter follows closely the associative habits of the language, and the existence of such inter-association habits are, therefore, of prime importance in making possible a large range of inner speech in reading. Chapter VIII is entitled, "The Interpretation of what is Read, and the Nature of Meaning." On the whole, meanings are usually felt as belonging to the larger wholes. Words are felt as having a part in the total, but their function is mainly to help tide one over to a place where a new meaning is suggested or completed. Imagery is a part always secondary or auxilary to the suggestion and control of meanings. The consciousness of meaning itself belongs in the main to that group of mental states, the feelings, which seem unanalyzable, or at least have a large unanalyzable core or body. The fact is that meaning is part and parcel of word-sound and word utterance, that is, what we take for the latter

two is largely word-meaning. It is safe to say that meanings in reading are mainly feeling reactions and motor attitudes attaching most intimately to, or fused with, the inner utterance of the words, and especially of the sentences, that are read. Chapter IX treats of the rate of reading, and the factors which condition speed. While the difference in reading time from page to page is small with individual readers, each falling into a reading pace most natural to him, the rate of reading varies greatly with different individuals. Experiments by Huey and Dearborn show that there can usually be much improvement. The thing to do is to make an effort to get away from our usual plodding pace, to read persistently as fast as possible and with well concentrated attention.

Part II, embracing chapters X to XIII, discusses "The Beginnings of reading, in the interpretation of gestures and pictures," "The Evolution of an alphabet and of reading by alphabetic symbols," "The Evolution of the printed page," and "The History of reading methods and texts."

The pedagogy of reading is taken up in the next six chapters (Part III) which are given to the following topics: "Present-day methods and texts in elementary reading," "The Views of representative educators concerning early reading," "Learning to read at home," "Learning to read at school," "Reading as a discipline, and as training in the effective use of books," "What to read; the reading of adolescents."

In part IV, treating of the hygiene of reading, "Reading fatigue" and "Hygienic requirements in the printing of books and papers" are discussed.

In the concluding chapter Dr. Huey takes up the future of reading and printing, showing possibilities of improvement which have never been canvassed, and for the elimination of waste. What we need now is more of particular researches on specific problems to furnish us with yet more of fact and of suggestion. M. W. MEYERHARDT.

Das Pferd des Herrn von Osten (Der kluge Hans), ein Beitrag zur experimentellen Tier- und Menschen-Psychologie, von OSKAR PFUNGST. Leipzig, Johann Ambrosius Barth, 1907. pp. 193.

It is rare that an experimenter is able to make a notable contribution, in a single piece of work, to both human and animal psychology, but that Dr. Pfungst has done in the case of a baffling problem by acute observation and a systematic application of the experimental method. The situation was briefly this. In 1904 there appeared in Berlin a remarkable reckoning stallion, the property of a retired schoolmaster living in one of the meaner quarters of the city, and making no effort to profit by the exhibition of his property. The horse was able besides doing other wonderful things to indicate by taps of his hoof the answers to problems involving the usual rules of simple arithmetic, including fractions, and to do this not only in the presence of his master, but also when the latter was absent and the problems were proposed by others whom it was quite impossible to suspect of fraud or collusion. Public interest in the matter was considerable and different sections of the public passed characteristic judgments upon the case. One party declared the case nothing but a piece of clever trickery; another held it to be a definite demonstration, at last, that animals could reason; while a third saw in it something occult, perhaps a case of telepathic transference of the results of the calculations from the mind of the master to that of the horse. The old schoolmaster himself declared that he had simply taught the horse arithmetic by regular pedagogical methods and that the horse had learned as children learn. Public interest ran so high that an in-

formal committee, composed of a number of distinguished scientific and practical men, made an investigation and published a report. In this they completely exonerated the schoolmaster of prompting the horse by intentional signals, and stated the opinion that unintentional signals of known sorts were absent. From this last statement the public promptly inferred that the committee meant to say that the horse did his own thinking, disregarding the remaining possibility that his behavior was regulated by unintentional signs of a sort as yet unknown.

At this point a systematic experimental attack upon the problem was begun by Professor Stumpf and his pupils which presently led to Dr. Pfungst's discovery of the set of minimal unconscious movements of the body and head by which the horse was set to tapping and again stopped at the number required. Once discovered, Dr Pfungst was able not only to execute these movements voluntarily (like the others who were able to get replies from the horse he had been executing them long unconsciously before he discovered them) and so lead the horse to any reply to any question, but he was able also to guide others to the observation of them in all those who succeeded in getting replies from the horse, and finally to demonstrate on a number of subjects in the laboratory similar and equally unconscious movements.

With this clue it was possible to explain in detail all the essential phenomena observed, even to the characteristic 'blunders' and 'mistakes' of the horse, and to give a natural explanation of how such a system of unconscious signalling by the master and of response by the horse could have grown up in the course of such "education" as the horse had received at the hands of Herr von Osten.

The explanation is doubtless absolutely correct and yet who would have ventured beforehand that differences in attention could explain so much — the delicate responsiveness of the horse to the movements of his master when his securing of bread and carrots depended upon it, and the obliviousness of the master to these same movements when his thoughts were otherwise engaged. The study is, all in all, a model in its field, and ought to be promptly translated into English.

For the details the text itself will, of course, be consulted. It includes, besides the six chapters of Dr. Pfungst, an introduction by Professor Stumpf, four appendices (on Herr von Osten's method of teaching, the report of the above mentioned committee, extracts from its protocol, and the final report made by Professor Stumpf in December, 1904) and a bibliography of 124 titles. E. C. S.

Are Bees Reflex Machines? An Experimental Contribution to the Natural History of the Honey Bee, by H. v. Buttell-Reepen, Ph. D. Translated by Mary H. Geisler. The A. I. Root Co., Medina, Ohio, 1907. pp. 48.

It is a pleasure to welcome, in an English version, this account of bee psychology by a German master of it. The work appeared first as a series of papers in the *Biologisches Centralblatt*, Vol. XX, 1900, as a counter blast to Bethe's stimulating but inadequate studies of ants and bees (Bethe: Dürfen wir Ameisen und Bienen psychische Qualitäten zuschreiben? Pflüger's Archiv, LXX, 1898), but is controversial only in presenting vividly the contrary facts. The work is full of interesting first hand observation with abundant references to literature and cannot fail, in its present form, to be useful to English students of bee behavior.

The difficulties and defects of psychological terminology are considerable at the best, but in comparative psychology they stand out in all their enormity. Hardly anywhere else would one meet in the same

paragraph a pair of sentences like the following: "On the basis of my observations I am of the opinion that bees possess either no consciousness at all, or one of only the lowest degree of development." . . . "We see that bees show signs of an admirable memory in their orientation, and also in other activities; further, I believe I have shown that the bee possesses a perception for color and form, and develops a rich capacity for communication . . . that, further, it is able to gather experience, to learn, and to form associations of impressions, etc." It is perhaps fair to say that Dr. v. Buttell gives us warning elsewhere that he does not always mean as anthropomorphic as he says.

The printing of the work in this English edition leaves not a little to be desired. Lines five inches long in type as fine as that used in the body of the pages is none too easy to read, and when it becomes still finer type in the footnotes it offers positive difficulties. E. C. S.

Biology and Its Makers, by WILLIAM A. LOCY, Ph. D., Sc. D. Henry Holt & Co., New York, 1908. pp. 469.

In this work Professor Locy of Northwestern University has given to the educated public an untechnical but scholarly account of the rise and present status of biology and its problems. The book will be welcomed both for its contents and straightforward, thoughtful style by students, clergymen, teachers, medical men, and all interested in the achievements of modern science. At the same time the broad outlook over the whole biological field,—comparative anatomy, embryology, histology, etc.,—the comprehensiveness and continuity of the work, its clear statement of problems, its excellent table of contents, index, and bibliography up to date will commend it to the specialist. For those who know a little about biology and wish to know more Professor Locy's volume is positively fascinating. One finds the theory of organic evolution fully and adequately treated, to be sure, but in its right setting as part of the orderly development of a great science. The cell theory, the discovery of protoplasm, the rise of bacteriology, and of the science of fossil life, and recent controversies in reference to germinal continuity and the inheritance of acquired characteristics are all presented in their just relation. The generous amount of biographical material, the portraits and other illustrations, and Professor Locy's appreciation of and almost religious respect for the great leaders of scientific thought are sure to make his book of very great educational significance. This work emphasizes the zoölogical rather than the botanical side of biological science as indeed might be expected by all familiar with the research work of the author.

Northwestern University. W. LIBBY.

La Mano. Appunti antropometrici et antropologici, del Dott. E. AUDENINO. Rivista Sperimentale di Freniatria e Medicina legale delle Alienazione mentale, Settembre, 1907. Vol. 33, pp. 416-429.

The author of this article is an assistant of Professor Lombroso at the psychiatrical and neuropathological clinic at Turin, and summarizes a number of previous studies of the hand as well as giving the results of his own investigations. The palmar lines of various simians have been studied by Alix, Andreoli, Morselli, Carrara and others, and certain characteristic longitudinal, and in some species, transverse lines, have been made out. In man, these transverse lines tend normally to become more oblique, but both they and the longitudinal lines appear as atavistic phenomena. Dott. Audenio compared the frequency with which these lines appear in normal man, in cretins, insane patients and epileptics with the following results: In normal man about 21% of the hands examined (87 men and 13 women) showed more or less complete longitudinal lines, the frequency being greater

among women than among men; in cretins these lines were found in 66% of the cases examined (39 in number), usually crossed by transverse and oblique lines so as to form an intricate network, a disposition rarely found in the hands of the insane; in idiots (20) and insane patients (20) longitudinal lines were found in 45% and in born delinquents, moral irresponsibles and epileptics (44) in 52% of the cases. The most frequent longitudinal line is that extending to the base of the middle finger, that extending to the base of the third finger is less frequent, and that extending to the base of the little finger occurs most rarely.

The form and proportion of the different parts of the hand have also been studied and the following have come to be considered simian characteristics, a very short thumb, the middle finger much shorter than the length of the rest of the hand (as in many of the idiots studied by Morselli and Tamburini), the index of the hand in comparison with the stature higher than 11-12, *i. e.*, the height more than 11-12 times the length of the hand. On this point researches have been made by Hovelaque, Hervè, Quetelet, Pruner, Bey, Lombroso and Marro, both upon simians and among different human races. They found the following indices:

Gorilla,	14.5	Chinese,	12.8	Australians,	11.9
Chimpanzee,	18	Berbers,	11.1	Todas,	11.8
Orang,	20.8	Palmese,	10.4	White races,	10 (nearly)
		Javanese,	13	Arabs,	11.1
		Hottentots,	11.8	Japanese,	12.8

In epileptics Féré found an average index of from 11.5-11.7; in normal individuals Marro found it to be 10.6 and in delinquents 10.1-12.9. In regard to the greater length of the second finger as compared with the third there has been much discussion, but withont arriving at any definite conclusions.

The author examined the index in all of his subjects, normals, cretins, idiots, insane, epileptics and born delinquents, but found no great difference in the percentages of the various forms, save that a low index was frequent in cretins. The length of the thumb was also investigated, but without marked results, except that the very short thumb occurred with greater frequency in cretins, and the very long thumb among idiots and imbeciles. Other proportions of the fingers were also studied, but without obtaining definite results. The article is illustrated and the illustrations show excellently various simian and human types, making available in very compact form the results of much research.

THEODATE L. SMITH.

Rivista Italiana di Neuropatologia, Psichiatria ed Elettroterapia. diretta da G. d' Abundo, Professore Ordinario di Clinica delle malattie Nervose e Mentali nella R. Università di Catania, Vol. I, Fasc. 5, Maggio, 1908. pp. 48.

This new periodical, published monthly, is, as its name indicates, devoted to neuropathology, psychiatry and electrotherapy. The present number contains an article by Prof. F. Del Greco, Director of the Insane Asylum at Como, on "Incomplete Recoveries from Insanity," and calls attention to a class of patients who, having recovered a sufficient degree of stability and normality to be discharged from the asylum, are, nevertheless, not fully competent to cope with the complexities of the ordinary conditions of life, especially in cases where the struggle for existence is likely to press heavily upon them. Such patients, though apparently normal or nearly so under sheltering conditions, may, under an unfavorable environment, become dangerous to themselves and to others. Some of them seem to realize this and do not

wish to be discharged from the asylum. Dr. Del Greco makes a plea for the establishment of special colonies, a system which has already proved successful in Germany and France, where patients of this class may be under supervision, but not subject to the restraints of an institution.

The second article is a clinical report of a case of Jacksonian epilepsy with a detailed account of the findings of the autopsy.

Considerable space is devoted to book reviews and the number closes with a report of the first Congress of the Italian Society of Neurology, held April 8th-12th, at Naples. A résumé is given of the following papers presented at the Congress: Aphasia, by Prof. G. Mingazzini; Physiology and Pathology of the Frontal Lobes, by Prof. Bianchi; and the Structure of Nerve Cells, by Prof. Fragnito. Announcements for the Congress of 1909, to be held at Genoa, are also given as follows: Prof. Catola will discuss Acute Myelitis from the clinico-experimental point of view; Prof. d'Abundo, the Physio-pathology of the Optic Thalamus; and Prof. Moreschi, Serum-diagnosis in Nervous Diseases.

THEODATE L. SMITH.

A Mind that Found Itself, an autobiography. By CLIFFORD WHITTINGHAM BEERS. Longmans, Green & Co., New York, 1908. pp. 363.

The author was a rather brillant graduate of Yale in the class of 1897, who, soon after leaving college and entering business had an attack of acute melancholia and threw himself from the fourth story window of his home, was taken to an asylum where after some three years he regained his equilibrium, was out for a few days and then alternated into a very exalted and maniacal state after which he very slowly recovered. His book is of very great value and interest from two points of view; first, because he has remembered with such detail the items of his delusions and can reproduce in a purely objective way his states of mind when insane and describes so vividly how gradually everything cleared up. This is its pyschological part. In this sense the book is only slightly like that describing the recovery of Guillaume Monod who remembered although he still accepted in later years a few of the insanities of his morbid period. The second end achieved by this book and perhaps the chief one intended by the author is a criticism of the present management of our insane asylums, chiefly, though not entirely, the private ones. The evil lies first of all with the assistants who are often ignorant, utterly untrained, passionate, vindicative and cruel, subjecting patients sometimes to endless humilations and outrage which they conspire not to see and to conceal from the physicians in charge. Sometimes physicians themselves indulge personal spites and dislikes and often, in asylums the heads of which have made great pretence of the "no restraint system", use it in some cases in its very worst form. To be strapped so tight that even the fingers cannot be moved and that every breath comes hard, so that the clothes cannot be pulled up at night and to be left to moan in pain for twelve hours for a series of days is an inhumanity that ought to be abolished. These things this author suffered and, as we may well believe him, to his detriment. If his book contributes to effect a reform in this respect alone, it will do great service.

Mental Pathology in its relation to Normal Psychology: A Course of Lectures delivered in the University of Leipzig. By G. STOERRING. Translated by T. Loveday. Swan Sonnenschein & Co., London, 1907. pp. x, 298.

Professor Stoerring's *Vorlesungen über Psychopathologie* first appeared in 1900, and were at once welcomed by students both of normal and of abnormal psychology. The book met a real need, and

certain portions of it—especially the discussion of psychological method in Lect. i.—have, by their frequency of quotation, become in a minor way almost classical.

The translator "found himself hampered by the lack of any English work on mental pathology" to which he might refer students of psychology "without embogging them in a morass of clinical details." He, therefore, undertook the present translation, in the conviction that Dr. Stoerring "offers the student a judicious selection of cases and, as a rule, exercises a cautiousness in interpretation that may serve as a wholesome corrective of the extravagances characteristic of much recent amateur work on abnormal mentality." In the reviewer's belief, this favorable opinion is justified. The work of translation has been well and conscientiously done, so that the volume may safely be recommended for class-work in colleges and universities.

P. E. WINTER.

Insanity and Allied Neuroses. By G. H. SAVAGE and E. GOODALL. With 6 colored plates and 45 illustrations in the text. New and enlarged edition. W. T. Keener & Co., Chicago, 1907. pp. xiv, 624.

This little work, a member of a series of Clinical Manuals for Practitioners and Students of Medicine, was first published in 1884; republished in revised form in 1890; and, after several reprintings, has now been issued in a third, revised and enlarged edition. It is a practical and clinical manual, based on the authors' experience in the Bethlem Royal Hospital and the Joint Counties Asylum, Carmarthen, and is directly addressed to the medical student. Special attention has been given, in the present edition, to the sections which deal with pathology; and the six colored plates, showing the gross and fine appearance of the brain in certain pathological conditions, appear for the first time. The concluding chapters, dealing with the legal relationships of the insane and with the provisions of the lunacy act of Great Britain, will be of interest for comparative purposes to the American reader.

P. E. WINTER.

Hypnotic Therapeutics, by JOHN DUNCAN QUACKENBOS. Harper & Bros., New York, 1908. pp. 340.

Seven years have passed since the appearance of the author's "Hypnotism in mental and moral culture" and he now records the results of his experience which has been an unusually rich one during these years. He believes that by hypnotism and suggestion he can cause sleep, restore for a time at least those near death, that he can often cure pronounced cases of neurasthenia, delusions, obsessions, morbid fears and propensities such as kleptomania, mania for lying, imposture, and can even correct moral perversion, cure love when it reaches the intensity of mad infatuation and correct erotomania, dipsomania, absinthism, tea and coffee inebriety, addiction to cigarettes, cocainism and stammering, and that he can cause singers and actors who are paralyzed by timidity to overcome their diffidence and succeed on the stage. Suggestion to him is a means of perfecting the pulpit orator, teacher, business man and typewriter. Even literary inspiration can be helped, and "psychics" has its place in pedagogy and in the home.

Religion and Medicine, by ELWOOD WORCESTER, SAMUEL MCCOMB and ISADOR H. CORIAT. Moffat, Yard & Co., New York, 1908. pp. 427.

Of this book, with its twenty chapters, Dr. Worcester writes seven, Dr. McComb five, and Dr. Coriat six, with two jointly by the first two authors. The work, as a whole, represents what has come to be

known as "The Emmanuel Church Movement," which began nearly three years ago. Dr. Coriat's chapters, save the last, have little, if indeed any, relation to this special work, and are extremely elementary. The other writers believe that the church is now likely to enter upon a new epoch, in which the healing ministry of Jesus, which has almost been forgotten, is to be revived in a practical way; and this is a movement, which in its practical importance they think comparable to the development of the higher criticism on the theoretic side. For the psychologist who is at all familiar with border-line phenomena there is little new. The authors, for obvious reasons, cannot describe in great detail the cases that come to them. They take the wise precaution of having all the important cases first diagnosed by a physician, and thereby avoid treating those due to serious organic lesions or to infection. The real significance of the movement, which we deem great, is thus not at all in its scientific character, for it has nothing essentially new to contribute to the psychologist, but in the fact that it may mark a new departure in the church which will set a wholesome back-fire to Eddyism, which has swept into its ranks so many intelligent men, and especially women, who ought to have been safeguarded against the wilder extravagances of this strange phenomenon of our day. We are glad to know that similar movements have been taken up by churches of other denominations. We cannot forbear to add that it would be an even more wholesome tendency if something of this topic were taught in every theological seminary, for it is a significant fact that the only profession devoted to saving souls, generally teaches clergymen in its seminaries nothing whatever of psychology, which seeks to tell what the soul is and does.

The Physical Phenomena of Spiritualism, by HERREWARD CARRINGTON. Small, Maynard & Co., Boston, 1908. 426 p.

This book is a godsend to all those interested in spiritualism. Three hundred and twenty pages are devoted to fraudulent manifestations, and only about one hundred to what the author deems genuine residua. Under the latter he classes raps, telekinesis, the mediumship of Home, levitation, elongation, fire tests, and the trance states of Mrs. Piper. These latter the author admits that he takes from literature, and it is a little difficult to understand why he claims to be a spiritualist. His explanations of the ordinary tricks such as table-tipping, lifting, rope tests, paraffin moulds, sealed letter reading, spirit photography, spirit posts, sacks, handcuffs, and, best of all, his explanation of single and double, prepared and unprepared slate methods, are admirable; so is his section on the psychology of deception and on test seances. It is obvious that any one who investigates these phenomena will at once discredit most so-called spiritual phenomena. To our thinking, the author needs only to investigate a little more than he has done the tricks that the mind plays on itself, in order to understand that all those phenomena which he accepts are to be explained in the same way.

Behind the scenes with the mediums, by DAVID P. ABBOTT. The Open Court Publishing Company, Chicago, 1907. pp. 328.

The author gives here a very interesting account of his attempts to test spirit mediums with reference, for instance, to Bishop's concealed letter reading, trick envelopes, a letter filched from the pocket, a rapping hand, the oracle of the swinging pendulum, the use of telepathy, the spirit message on a slate, flower materialization, spirit voices, taps, lights, luminous costumes, various modes by which mediums read concealed letters, the different kinds of manipulation, etc. This will suffice to indicate the content of the book. It is a valuable unmasking and should be in the hands of every one interested in the subject.

Abriss der Psychologie, by HERMANN EBBINGHAUS. Veit & Co., Leipzig, 1908. pp. 196.

This little outline, which ought really to be translated into English, begins with a brief history of psychology. Then follows in the first section short chapters on the relation of brain to soul, structure of the nervous system, reaction and parallelism, nature of the soul. The second division is on elementary phenomena of psychic life—sensations, concepts, feelings, instincts, will. Then the fundamental laws of psychic processes—attention, memory, fatigue. Third, the outer effects of these processes seen in sensation and movement, concept and movement. The third division is entitled "The Complications of Psychic Life." Under the caption of the life of concepts, the author treats perception, illusions, memory, abstraction, with twenty good pages on language; then follows a discourse on the relations between that and knowledge and faith. The second part of the third section deals with feeling and action. Here first are discussed the causes of the complications of feelings—their intensity, form, association, irradiation, the passions and moods, and complex and free activities. The last section is devoted to the highest activities of the soul—the disadvantages or the evils of foresight, religion, art and morals. We only wish the book had been more copiously illustrated than with the very slight seven cuts.

Goethe's Wetzlarer Verwandschaft, von R. SOMMER. Leipzig, J. A. Barth, 1908. pp. 47. Mit 8 Abbildungen.

The argument of this little book is as follows. Goethe tells us that he had from his mother "die Frohnatur und Lust zu fabulieren." Since Goethe closely resembles in feature and expression his mother's mother, the question arises whether the "Lust zu fabulieren" did not come from his maternal grandmother. Of her, the Frau Stadtschultheiss Textor geb. Lindheimer, we have no record save a scandalous and untrustworthy statement of Senckenberg's. Of her father, Goethe's maternal great-grandfather, we know more: he was part author of a satire, "Diarium obsidionis Wetzlariensis," 1702,—though how much he wrote of this satire is unknown: the chief author was apparently a Dr. von Pulian. The satire (it is printed by Dr. Sommer) shows five characteristics: clearness of visual imagery, fanciful embroidery of real events ('confabulation'), pleasure in the drastic and grotesque, an underlying serious appreciation of cultural conditions, and a rationalistically pedantic and lawyer-like style. As all these characteristics may be found in Goethe's writing, we may conclude that Goethe owed the "Lust zu fabulieren" to the Lindheimer stock.

The weak points in the argumentation are clear enough. We know nothing of the temperament of the maternal grandmother; the satire was composed by "einige Wetzlarische Witzlinge" of whom Cornelius Lindheimer was seemingly not the chief, so that his part in its composition may have been small; the main characters of the satire, which is by no means underestimated by Dr. Sommer, are the characters of satire at large; and von Pulian was a lawyer, so that the 'Witzlinge' would naturally belong to his circle, and the legal style is accounted for. One would say that the positive evidence must be strengthened, and that there must be negative evidence (marked absence of certain attributes in the other lines of descent) to support it, before Dr. Sommer's conclusion can be accepted. P. E. WINTER.

The Riddle of the Personality, by H. ADDINGTON BRUCE. Moffat, Yard & Company, New York, 1908. pp. 247.

Much of this work originally appeared in Appleton's Magazine and

this sufficiently indicates its somewhat popular character. The writer discusses the subliminal self, the relations of the subconscious to American explorers, pioneers in France and the New World, the evidence for survival, the nemesis of spiritism, D. D. Home and Usapia Palideno, a census of hallucinations, hypnotism and the drink habit, hypnoidization, spiritism and telepathy, and the work concludes with hints for further reading. The author states that he has received "personal counsel and aid from Professor James of Harvard and from Professor Hyslop of the American Institute for Psychic Research and from others." The author thinks that such studies provide a corrective for the conclusions toward which the investigations of the psychopathologists tend, despite the fact that they have done much to alleviate human suffering. It is sufficient characterization of the work to say that the author is in quest of proof of a future life and thinks he finds it from these studies.

Plato's Psychology in its Bearings on the Development of Will, by MARY HAY WOOD. Harry Frowde, New York, 1907. pp. 62.

This master's thesis is really a valuable addition to the apparatus of the student of Plato. The author gathers his general views about mental activities, whole and part concerning appetite, emotion, thought, reason, will, and brings the various important passages together under each of these heads, showing excellent knowledge of the author in the original.

Modern Classical Philosophers. Compiled by Benjamin Rand. Houghton, Mifflin & Company, Boston, 1908. pp. 740.

This book presents in a series of extracts some of the essential things of the chief philosophic systems from Bruno to Herbert Spencer both inclusive. It is a history of modern philosophy based upon selections from the original text which are translated from the original into English. The author has sought to apply the case system used in teaching law. The work seeks to enable the reader to discover at once the content and method of the great and philosophic masters of modern times. The writers included are Bacon, Hobbs, Descartes, Spinoza, Liebnitz, Locke, Burke, Hume, Condillac, Kant, Fichte, Hegel, Schopenhauer, Comte, John Stuart Mill.

Lucretius, Epicurean and Poet, by JOHN MASSON. John Murray, London, 1907. pp. 453.

This book is really a Godsend to all teachers of the history of philosophy. It is comprehensive and learned and treats of the times of Lucretius; his life, his influence on his own age; the origin of the atomic theory; the atom of Lucretius; the birth of the world; how modern science bridges over the gulf between atoms and living things, the controversy as to the potency of matter; Epicurean psychology; atomic declination and free will; the theory of images; the Epicurean gods; the world as conceived by Lucretius; the roots of Epicureanism, Democritus; Epicurus as an ethical teacher; poetry and science; what the world owes to Lucretius; his teaching and personality.

The Will to Doubt: an essay in philosophy for the general thinker. By ALFRED H. LLOYD. Swan, Sonnenschein & Company, London, 1907. pp. 285. (Ethical Library.)

Truth, the author premises, has neither visible form nor body, is without habitation or name, like the Son of Man it hath not where to lay its head. This work is designed to be in some sense an introduction to philosophy, although it is really addressed quite as much to the general reader. The author seeks to meet a real emergency of the

day, namely, the doubt that is now so much abroad and fraught with so much danger and the evil effects of which are so often charged up against universities. His thesis is that doubt is essential to real belief. Hence his first chapter is the confession of doubt and then follows an account of the difficulties in the ordinary view of things, especially that of science since its rise. Its limitations are thought to be objective, specialistic, agnostic, although there may possibly be value in these essential defects of experience. The writer discusses, too, the personal, the social, the vital and the formal in experience. Descartes is taken as an early modern doubter. The most elaborate chapter is a characterization of the doubter's world, which is reality without finality; while really there is perfect sympathy between the spiritual and the material and a genuine individuality culminating in immortality. The last theme is doubt and belief. There is no closer approximation to the very basis of duty than the principle: Whatever is is right; and this is the summing up of the whole matter.

Erotische Æsthetik, by ERNST SUBAK. Ernest Hoffman, Berlin, 1908. pp. 79.

To this curious mind, the psychic activities are the highest form of an æsthetic sex function. The pleasure of procreation is irriadiated into and diffused through all art and even science, and every form of knowledge which is penetrated by any degree of interest and appreciation. Love in ascribing worth to a person does so to conserve the lover's own ego. Æsthetic appreciation is a sex function of the brain. Music, fine arts, philosophy, categories of knowledge of the external world, are explained so simply and beautifully and easily that it really is a marvel that it was left to our late day and to this youthful writer to explain all the problems of the noetic world at once by his "erotic æsthetics."

L'Idéal Moderne: La Question Morale—la Question Sociale—la Question Religieuse, by PAUL GAULTIER. Hachette & Cie, Paris, 1908. pp. 355.

The writer divides his exposition of the subject into three parts: moral, social and religious. In the first, he shows its independence, describes the renaissance of the antique ideal and defends individualism. In treating social morality he lays down the laws of public and private charity and solidarity, for justice and injustice, for the social antinomy that arises between liberty and equality, and finds its solution in liberal socialism. He then discusses the relations between religion and morals, and between science and faith, with a history of the latter and of revelation; and to conclude, characterizes the religion of the modern spirit. He retains the point of view of what he calls integral spiritualism.

Morals: a treatise on the psycho-sociological basis of ethics, by G. L. DUPRAT. Translated by W. J. Greenstreet. The Walter Scott Publishing Company, London, 1903. pp. 382.

The writer first treats of the method, discussing under its caption ethics, metaphysics, religion, scientific morality and the modes of ethical research. In the second part, under the caption of the psychological ideal, he treats the moral will, liberty and morality, the moral tendencies, the moral individual, determinism and immoral actions. Under the social ideal, he treats of social evolution, rights, the state, the economic organization, the family, friendship and the collective sentiments; and under part four, the struggle against immorality, the ethical sanction and moral education. As a whole the work is somewhat abstract, but it breathes the spirit of Ribot, Paulhan, Janet, Ferri and others.

A Manual of Ethics, by JOHN S. MACKENZIE. 6th impression. W. B. Clive, London, 1907. pp. 472. (University Tutorial Series.)

This is a fourth edition with no very great changes or enlargements. It is a systematic and comprehensive work dealing with the scope of ethics, its relations to other sciences, its divisions, desire, will, motive, intention, character, conduct, moral judgment, ethical thought and theory, various standards, virtues, moral life and institutions, social unity, duties, moral pathology and progress.

Philosophia Militans, by FRIEDRICH PAULSEN. Reuther & Reichard, Berlin, 1908. 233 p.

The author here prints eight recent papers, the chief of which are: The newest heresy condemnations in the field of modern philosophy; Kant the philosopher of Protestantism; Catholocism and science; Modernism and the encyclical of Pius X; Fichte in war for the freedom of thought; Hæckel as a philosopher, his world riddle as a folk book; and The Discovery of man in the nineteenth century.

Psyche und Leben, von W. v. BECHTEREW. 2nd ed. J. F. Bergmann, Wiesbaden, 1908. pp. 209.

In this second edition the author amplifies his views, which may be summarized somewhat as follows: Energy is primarily an active state of imponderable small particles, and when these break up there is no trace of anything physical left. The psyche may be a result of the transformation of energy so that it is not radically different from substance itself, so the ideas matter, energy, and psyche admit of being conceived in a unitary manner. Thus energy is intermediate between the material and the psychic world and has nothing originally physical about it. In its essence the psyche, like life, is neither movement nor energy, but in only both potentially. It can be transformed into outer material or inner subjective objects. Thus the entire world is the expression of unitary energy which includes potentially the psychic. This gives us the basis of the new parallelism and makes the world again a real unity.

Wunscherfüllung und Symbolik im Marchen, by DR. FRANZ RIKLIN. Deuticke, Leipzig, 1908. pp. 96.

The soul tends to work over the world towards conformity with its wishes. This is especially the case when thought is not in the closest contact with external reality; for instance, in dreams and revery. Poets create in fantasy what life denies them. Gottfried Keller was not a favorite with women, yet developed the highest ideal types of them; so Johanna Spyri described one of the most perfect children when deprived of her nephew. And so in countless cases, the wish creates a fancied substitute for reality and the tendency to identify the imagined object with experience is based on the deep instinct toward wish fulfillment. There are wish dreams and deliria, by prisoners, of freedom. Insane delusions are sometimes only desires expressed naïvely without critical reductives. Dead relatives who are intensely mourned for may appear perhaps as angels or ghosts with messages of comfort, and thus nature seeks to heal psychic wounds. So in myths there are wishing caps, love potions, seven-league boots, strength shirts and gloves, giants are overcome by magic weapons, there are miracles, mirrors that reveal all one wishes to know, riches, table delicacies, magic tables, etc. What does not at first seem the direct fulfillment of wishes becomes so when interpreted symbolically. Riklin has little difficulty in finding in mythology abundant sex myths to be interpreted as the Freud school interpret unconscious

constellations of ideas or wit. Thinking in symbols is a psychosis, an earlier culture stage; we see it often in dementia præcox and in various types of insanity. Animals, colors, jesters, angelic and demonic beings constitute a language of their own. General psychic states tend to incorporate themselves into specific forms and images, and this is most common in the submerged strata of the soul that represent a past stage of this evolution. Thus fire, genitals, weapons, beasts, are prominent in these early types and forms of thought. Transformations of men to animals and *vice versa* are symbolic. In all there is some transference upward or downward as shown by the method of diagnostic associations. Everything has hidden meanings. Thus in all such phenomena, we are really studying the psychic processes of primitive men, our very remote human ancestors.

Das Recht über Sich Selbst, seine strafrechtsphilosophische Studie, von KURT HILLER. Carl Winter, Heidelberg, 1908. pp. 114.

This author has the courage to imply that man has a right to commit suicide, mutilate himself, and in general may, with greater justice than has hitherto been assumed, exercise control over his own life in those matters which do not affect directly the well-being of others. This claim, of course, he does not make absolute, but discusses the American and other forms of duel, the murder of those who desire to be killed, homo sexuality, bestiality, abortions, etc. The author subjects to a rather gruelling discussion the norms which the German penal code uses to define the right of the individual to dispose of himself as he sees fit. He admits his work is in the nature of an isolated speculation and is based on the rather vague conception of natural rights. He says we thus reach the conclusion that "all the arguments for the legal punishment of those slain by their own consent, incest, pediastry, bestiality, and abortions are insufficient, and their penalization is inconsequent." The right of the individual to autonomously dispose of his own personality is now more and more reduced. All admit that he must not harm others, but no one can justify the interference of the State where this does not occur. Where others are harmed very slightly, there ought to be modifications, and the penal norms should be reduced. He admits that this is radical.

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BOOK NOTES.

Die Cerebrale Sekundärfunction, von OTTO GROSS. F. C. W. Vogel, Leipzig, 1902. pp. 69.

Every nervous element that is functionally excited tends to persist in this state after the first cause has ceased to act. In respect, however, to this after or secondary function, individuals differ very greatly. Mentality is largely built up upon the intensity and duration of these after effects. If these are reduced and the excitability of the primary function increased, there is danger, and this is seen in its most flagrant form in mania. Consciousness is largely the result of this secondary process, which gives us groups of concepts and themata which the mind works out. One trait of degenerative constitutions is that consciousness becomes superficial or narrow. If these changes of secondary function are acquired and too diffuse, acute psychoses result. When the disturbance is circumscribed, paranoia follows. Even eras and time periods differ in ways best described under the author's rubrics.

Die Melancholie: ein Zustandsbild des manisch-depressiven Irreseins, by GEORGES L. DREYFUS. Mit einen Vorwort von Hofrat Professor Dr. Emil Kraepelin. Review reprinted from the Journal of Philosophy, Psychology, and Scientific Methods, Vol IV, No. 25, Dec. 5, 1907, pp. 692-698.

F. L. Wells gives us an interesting review of this important book, from which it appears that the author has made a careful study of eighty-one cases and believes that his results undermine the decision of Kraepelin that melancholia is a disease entity. It is suggestive that the last named author accepts these results in his introduction. Dreyfus makes here the first extended application of the statistical method in this field. He has made the best use of a continuous diagnostic policy which has extended over many years. After treating the history of melancholia, he shows that as early as 1878, it began to be recorded as a disease of the involution period. He traces the evolution of Kraepelin's views through the different conditions of his work, one main point of which is stressing the diagnostic value of retardation. In general, the involution melancholias are losing their integrity in the larger maniac-depressive group. From the depression of this group, however, it is distinguished by the presence of anxiety and agitation, and especially by the absence of retardation which is a factor of great and growing importance.

L'aphasie de Broca, by FRANÇOIS MOUTIER. G. Steinheil, Paris, 1908. 2 vols.

In these ponderous tomes, the author has discussed the history of the aphasia of Broca and its cerebral localization. Under this head, he treats of the anatomical documents of Broca; the observations, favorable and classic, in support of, and those contrary to, his theory of localization; the supposed syndromes of transcortical motor-aphasia; the theory of localization on the left side; the distribution of sense about the Sylvian fissure; anatomical dissociation and localization, according to Pierre Marie; the lenticular zone and that of Wernicke; the new localization according to recent studies. In the

next part, he discusses clinically the nature and symptoms of aphasia, beginning with the classical conception of Broca; he then takes up anarthria, clinical descriptions and the intellectual defect of aphasic patients; errors in diagnosing between dementia and aphasia; criticisms made by the new clinical studies of Broca; symptoms and verbal images. In the appendix, pages 251-769, the author prints justificative monographs from 1861 down; and from page 375 on gives observations that he has made himself. The work terminates with a bibliography, a lexicon of terms and a table of authors.

Prognosis in Cases of Mental Disease Showing the Feeling of Unreality, by FREDERIC H. PACKARD. Rep. from American Journal of Insanity, Vol. 64, No. 2, Oct., 1907, pp. 263-269.

In the allopsychic field, such expressions as "Things do not look real," in the somatopsychic field, the feeling that hands are changed, organs gone, etc., in the autopsychic field, that "I am not I, am dead," that "This is not Mr. So and So," are the three forms of a failing sense of reality. Packard thinks that all these deliriums of negation are due to anxious melancholy, and that the feeling of unreality is not an essential symptom of involution melancholy. He holds that this fantastic symptom should not be given too much importance, and that both diagnosis and prognosis should be made on the basis of more fundamental symptoms.

Heredity, by J. ARTHUR THOMSON. J. P. Putnam's Sons, New York, 1908. pp. 605.

This book has been long awaited with interest and will be read with satisfaction. Its author is, perhaps, the most holophrastic of all the able disciples of Weismann who write English. In the first chapter he defines and illustrates heredity and inheritance; in the second, discusses the physical basis of inheritance; in the third, heredity and variation; in the fourth, common modes of inheritance; fifth, reversion and allied phenomena; sixth, telegony and other disputed questions; seventh, transmission of acquired characters; eighth, heredity and disease; ninth, statistical study of inheritance; tenth, experimental study of inheritance; eleventh, history of theories of heredity and inheritance; twelfth, heredity and development; thirteenth, heredity and sex; fourteenth, social aspects of biological results. There are forty-nine illustrations, tbe text is good, the matter well distributed, the bibliography copious, and the work is admirably calculated to take a leading place among the text-books on the subject.

Organische Zweckmässigkeit, Entwicklung und Vererbung vom Standpunkte der Physiologie, von DR. PAUL JENSEN. Gustav Fisher, Jena, 1907. pp. 251.

This is a portion of a larger forthcoming "Introduction to general physiology." It begins by a criticism of Darwin's theory of selection, of auto-genetic and allogenetic theories. The variability of our organism is next characterized, its different types are pointed out and their significance for phylogeny is shown and the causes of progressive variability are traced. Then follows a critique of the various idioblastic and chromosome and the bioblastic theories. The purposiveness of organisms is discussed and the difference between true and false teleology is pointed out. The hypotheses of Ranke, Driesch, Wundt, Cosmann are briefly described, The relations of the whole problem to the monistic theory are next taken up, and this is the basis of the author's development of his own scheme of both description and classification. This work is a valuable addition to the repertory of the student desiring to put himself abreast of modern theories upon this difficult subject.

Versuch einer Begründung der Deszendenztheorie, by KARL C. SCHNEIDER. Fischer, Jena, 1908. pp. 132.

The first stimulus in the lowest form of life takes us at once into the psychic sphere. Under the law of the conservation of the psyche, all that passes from parent to offspring is a mneme. The psyche is the chief biological function throughout the entire evolutionary history of life. The first human soul was an epochful and spontaneous generation, when an over-consciousness was born that can turn upon nature and overcome her. The "exonoetic" process of experience is the origin of man. Just so far as he has become man, his physical development has ceased.

The Application of Statistical Methods to the Problems of Psychophysics, by F. M. URBAN. The Psychological Clinic Press, Philadelphia, 1908. pp. 221. (Experimental Studies in Psychology and Pedagogy, edited by Lichtner Witmer.)

The first chapter is devoted to the descriptions of the experiments. Then follow chapters on the statistical numbers of relative frequency, the method of just perceptible differences, equality cases, the psychometric functions, and lastly a general inquiry concerning these latter.

Grundzüge des Physiologische Psychologien, von WILHELM WUNDT. Sechste, umgearbeitete Auflage, Erster Band, mit 181 Figuren im Text sowie Sach- und Namenregister; Leipzig, Verlag von Wilhelm Engelmann, 1908. pp. 725.

This initial volume of the sixth edition of Wundt's great work has not only been carefully revised but increased considerably in bulk. New sections have been added in the introduction and earlier chapters, and a most radical working over has been given to the ninth chapter, the last in this volume, which deals with the Intensity of Sensations. The purpose here has been to bring out more clearly the psychological aspects of the matters dealt with and to recast the discussion of the psychophysic methods. A more detailed notice of the changes in this chapter will be given in a later issue of the *Journal*. The volume is provided with special indexes of names and subjects by Wundt's assistant, Dr. Otto Klemm. E. C. S.

The Influence of Alcohol and other Drugs on Fatigue. The Croonian Lectures delivered at the Royal College of Physicians in 1906. By W. H. R. RIVERS. Edward Arnold, London, 1908. pp. 136.

The writer first gives a history of the current methods of studying muscular and mental fatigue and that of attention, tracing the effects of drugs, etc. Subsequent lectures are devoted one each to caffeine, the influence of alcohol upon mental fatigue, of the use of cocaine, strychnine, tobacco, etc., while in the appendix a typical fatigue curve is given and a new apparatus for the application of MacDougall's method of studying mental fatigue. Every student of psychology and education will be glad to have in this volume an interesting and comprehensive description of the best methods and results that have been brought out by the many modes of studying this subject during the last decade or two.

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NORMAL PERFORMANCE IN THE TAPPING TEST

BEFORE AND DURING PRACTICE, WITH SPECIAL REFERENCE TO FATIGUE PHENOMENA

By FREDERIC LYMAN WELLS, Ph. D., Assistant in Pathological Psychology in the McLean Hospital, Waverley, Mass.

This is an attempt to further standardize a psychological measure of a relatively high degree of simplicity, precision, and responsiveness. The measure is that of the maximum rate of repeated voluntary movements, as described and reviewed in a previous paper.[1] The taps are executed upon a telegraph key which is in circuit with a Porter signal magnet recording on smoked paper upon the 50 cm. drum of a standard Ludwig-Baltzar kymograph. The speed of the drum is approximately 13 mm. per second, the second intervals being directly recorded by a Jaquet chronograph adjusted to the pen of the magnet. The subject is left free to choose the preferred method of tapping, care being taken to see that the method is not essentially altered during the experiment. Attention has been called to sources of error in this freedom of procedure but these can hardly be obviated without introducing other and more important errors. Then too, an important object of the experiments was to obtain normal data for comparison with pathological cases unamenable to more rigid experimental control.

The conduct of the experiment is as follows. The subject begins at a given signal and taps at the maximum rate until a record of 30 seconds is obtained, when he receives a signal to stop. Then follows a 2′ 30″ pause, after which a second series is executed, then another 2′ 30″ pause and so on until five series of 30″, each with an intervening pause of 2′ 30″

[1] *American Journal of Psychology*, Vol. XIX, 1908, pp. 345-58.

have been obtained. A similar record of five 30″ series is then made with the left hand, and in repeated experiments the hands alternate in precedence from experiment to experiment. The subject refrained from muscular effort during the pauses, except that in the experiments in which the writer played the rôle of both subject and operator, the spring of the kymograph was wound with the other hand than that used for the tapping. The 2′ 30″ pause was practically a chance selection, and no attempt has been made to determine the relative influence on the test of pauses of shorter or longer duration.

The experiment is evaluated by counting the number of times the key is struck and the circuit closed during each of the six intervals of 5 seconds in the 30 seconds tapping. If, as sometimes happens, it can be established that the key was struck without closing the circuit, this is counted as a tap. The period of 5 seconds was selected because it has been that most frequently used by previous investigators of the test, and we know more about it in terms of 5 second performances than in any other terms. From the writer's results, which extend considerably beyond the material to be reported here, it is doubtful if any more efficient system of evaluation could be selected for the purposes in hand. To illustrate more clearly the precise method of dealing with the data there may be quoted in full the figures of a sample record with the right hand upon a normal individual, near the limit of practice.

	1st Interval 0–5″	2nd Interval 5″–10″	3rd Interval 10″–15″	4th Interval 15″–20″	5th Interval 20″–25″	6th Interval 25″–30″	No. of Taps in each Series
1st Series	41	37	35	34	34	32	213
2nd Series	41	37	36	35	34	34	217
3rd Series	40	39	37	37	35	34	222
4th Series	40	39	37	36	36	35	223
5th Series	41	39	38	37	36	36	227
Average of Intervals	40.6	38.2	36.6	35.8	35.0	34.2	

"Total Efficiency" of Record **220.4.**

Each of the thirty 2-place figures gives the number of taps executed in an interval of 5 seconds. Reading the top line from left to right we obtain a gradual decrease in the size of the figures, indicating a slowing up in the tapping rate. The right hand figure of three integers, 213, gives the sum of the preceding figures for the six intervals, *i. e.*, the number of taps for the whole 30 seconds. Each successive line may be read in the same way. Considering the figures in column, we obtain the figures of two digits and a decimal on the bottom line (40.6, 38.2, 36.6, 35.8, 35.0, 34.2) the average number of taps executed during the first 5 seconds, the second 5 seconds, etc. Naturally, a regular decrease is noted. The figure in heavy-faced type, **220.4**, gives the average of the five three-digit figures above it, *i.e.*, the average number of taps executed each time during the five 30 second series. This figure is taken as indicating the "total efficiency" of the function in that record, subject, of course, to the variability of the single series it represents. Subsequently, therefore, unless otherwise evident, a two place integer may be taken to represent a number of taps done in a single interval of 5 seconds, a figure of two places and a decimal to represent the verage of number of taps in five cases of a certain 5 second interval. A three place integer indicates a number of taps in a series of 30 seconds, a three place integer with a decimal indicates the average performance in five such series, the total efficiency of the record. Six successive 5″ *intervals* thus form a *series*, five 30″ series a *record*, and the two records of right and left hand constitute the single *experiment*.

The material upon which the present study is based consists of eighty such experiments. Two experiments each were made upon ten normal individuals, and thirty experiments each upon two individuals. One of the subjects in the thirty experiments was the writer; the other was Mr. F. W. Kinsman, a nurse in the hospital, the fidelity of whose co-operation is quite as evident in the figures of his results as it could be made by verbal assurances.

The experiments upon the ten normal subjects were made between 8 A. M. and 2.30 P. M., and the second test was made at the same time of the day as the first, one week later, except in the case of Subject H, in whom the intercurrence of a sprained wrist necessitated a longer interval. The practice curve of the writer consisted of thirty daily experiments broken by intermissions of two weeks after the 14th, and 10 days after the 24th and 26th days. The routine in the other subject was similar, save that his first two days are taken from the records of the ten normal subjects, of whom he was also one. The time of day was not so rigidly controlled as in the

ten subjects, but it fell between 7.45 and 9 A. M. for the former, and between 8 and 10 A. M. for the latter subject. There is evidence that no significant error is induced by this variation.[1]

Inasmuch as the material is to be considered from a number of often only indirectly related points of view, the scope of the discussion may be made clearer by the following enumeration of the points to be brought out. The portions of the study dealing with these various topics have corresponding headings and the main findings on each point are similarly summarized at the close of the paper.

1. Individual differences in "Total Efficiency."
2. Index of right-handedness.
3. Rate in the successive series, "warming up."
4. Rate in the successive intervals, fatigue.
5. Fatigue and individual variation.
6. Total efficiency practice for the two subjects.
7. Relative practice of right and left hands.
8. The effect of practice upon the "warming up" phenomenon.
9. Practice and the successive intervals.
10. The influence of intermissions upon practice gain.
11. The effect of "warming up," as brought out by practice, on the different intervals.
12. An "index of fatigue."
13. Correlation of the fatigue phenomena in the different hands.
14. The relative fatigability of right and left hands.
15. The correlation of fatigability and initial rate.
16. The effect of practice on fatigability.
17. Variability.
18. The subjective condition as related to gross rate and fatigability.

1. Individual Differences in "Total Efficiency." The following table gives for the ten subjects the gross rate in terms of number of taps in a series of 30 seconds, without reference to fatigue phenomena. The series for each hand are arranged in columns of five, according to their order in the record. The figures in heavy faced type give the averages of the figures above or preceding them, according as we consider the average rate of each subject, or the average of the first, second, third, fourth or fifth series in all ten subjects. Thus the aver-

[1]Since the practice of Subject I was necessarily with knowledge of immediate results, it was considered desirable that Subject II should also have an equal knowledge. The present practice is, therefore, "with knowledge of results," but only to the extent of a knowledge of the gross practice gain.

age of Subject E's, five series of taps with the right hand in the first experiment is **191.2**, in the second experiment is

TABLE I.

GROSS RATES IN TEN NORMAL INDIVIDUALS. NUMBER OF TAPS IN 30 SECONDS.

First Experiment, Right Hand.

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
1st Series	218	153	182	198	192	201	175	216	209	180	**192.4**	*16.0*
2nd "	217	156	175	197	187	195	187	209	201	181	**190.5**	*13.3*
3rd "	221	156	174	201	191	207	192	206	205	185	**193.7**	*14.6*
4th "	229	156	204	204	195	204	189	212	209	186	**198.8**	*13.8*
5th "	209	155	182	215	191	205	192	215	206	186	**199.2**	*14.4*
Average	**218.8**	**155.2**	**183.4**	**203.0**	**191.2**	**202.4**	**187.0**	**211.6**	**208.0**	**183.6**	**194.9**	
M. V.	*5.0*	*1.0*	*8.0*	*5.2*	*1.8*	*3.6*	*4.8*	*3.2*	*2.0*	*2.2*	*3.7*	

TABLE I. (Cont.)

Second Experiment, Right Hand.

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
1st Series	216	147	163	200	199	205	193	212	200	175	**189.0**	*20.9*
2nd "	225	150	158	202	193	202	193	210	204	175	**191.4**	*18.0*
3rd "	239	158	160	202	197	207	202	206	200	180	**195.1**	*17.5*
4th "	242	147	162	208	207	208	203	211	204	178	**197.0**	*20.8*
5th "	234	151	160	207	207	207	192	214	205	182	**195.9**	*19.7*
Average	**231.2**	**150.6**	**160.6**	**203.8**	**200.6**	**205.8**	**196.6**	**210.6**	**202.6**	**178.0**	**193.7**	
M. V.	*8.6*	*3.2*	*1.6*	*3.0*	*5.2*	*1.8*	*4.8*	*2.0*	*2.0*	*2.4*	*2.5*	

First Experiment, Left Hand.

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
1st Series	200	132	159	185	167	163	173	198	181	171	**172.9**	*14.3*
2nd "	202	127	161	175	166	166	185	186	185	170	**172.3**	*14.3*
3rd "	206	131	159	171	179	173	170	188	188	174	**173.9**	*13.1*
4th "	203	123	162	173	174	166	175	191	190	167	**172.4**	*14.4*
5th "	199	120	152	176	177	176	177	200	188	168	**173.3**	*16.1*
Average	**202.0**	**126.6**	**158.6**	**176.0**	**172.6**	**168.2**	**176.0**	**192.6**	**186.4**	**170.0**	**173.0**	
M. V.	*2.0*	*4.0*	*2.4*	*3.4*	*4.8*	*3.8*	*4.0*	*5.2*	*2.8*	*2.0*	*3.4*	

TABLE I. (Cont.)

Second Experiment, Left Hand.

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
1st Series	209	138	153	188	187	177	172	197	191	170	**178.2**	*16.8*
2nd "	207	135	159	177	185	167	169	194	190	165	**174.8**	*15.8*
3rd "	221	127	155	180	185	179	171	197	191	167	**177.3**	*17.7*
4th "	213	128	155	169	198	174	174	196	189	163	**175.9**	*19.4*
5th "	220	133	154	177	182	174	168	191	188	165	**175.1**	*15.7*
Average	**214.0**	**132.2**	**155.2**	**178.0**	**187.4**	**174.2**	**170.8**	**195.0**	**189.8**	**166.0**	**176.3**	
M. V.	*5.0*	*3.8*	*1.4*	*4.6*	*4.2*	*3.0*	*1.8*	*2.0*	*1.0*	*2.0*	*2.9*	

200.6, but the average number of taps of all subjects during the first series for the right hand is **192.4** in the first experi-

ment and **189.0** in the second. Let us first consider the gross rates of the various subjects as given in the heavy faced figures running horizontally across the table.

It is difficult to estimate the range of individual differences because the limit is not well defined. The rates of rhythmic discharge in tonic contractions obtained by Schaefer and others have been surpassed in repeated voluntary movements. The fastest initial rate on record is, I believe, about 15 per second. Our rates here are much slower, the fastest and slowest subjects, who happen to be the first two quoted, vary as about 3:2. With a greater number of subjects, we should doubtless have increased this range.

It will be remembered that Bolton, working with large groups of children found a group correlation between a general superiority of make-up and the gross rate of tapping. Bagley did not find it for class standing. What correlations in this respect may obtain for groups, the present observations are not of a character to say, but as between individuals they are altogether insufficient to serve as a basis for deductions as to any phase of physical or psychical constitution. W. G. Smith[1] mentions that among his fastest subjects were epileptic dements; the fastest record but one obtained by the writer is from a well developed case of general paralysis. The writer has observed a few individuals who, tapping unusually fast, without special practice, were of highly "nervous" temperament, but equally "nervous" temperaments may be found among those whose maximum rate is below the average.

What is the precise physiological significance of the maximum rate is by no means well made out, and cannot be deeply entered into here. It seems to be generally conceded that it is limited by the refractory phase of the synapses in the motor pathways, but that does not make the tapping test a measure of the period of this refractory phase; at least not in the earlier stages of practice. It is probable that the measure of this period would give us data of considerable psychological significance, and it may be possible to study it through the methods described by Schaefer and others. In the later stages of practice we probably do obtain in the tapping test a measure greatly analogous to this, but in the beginning, as we ordinarily have to apply the test, the factors in speed are probably those of co-ordination mainly,[2] and cannot be expected

[1] *Br. J. Ps.*, I, 256.

[2] In so far as the graphic record of the manipulation of the telegraph key affords an analysis of the tapping movements, it brings out a rather interesting point with respect to the nature of this co-ordination. The record gives of course, the time during which the key is held down distinct from that during which it is released. In Subject

to afford information about the condition of the motor pathways as given in the refractory phase. A high gross rate in tapping does not, as has been pointed out, involve superiority in other aspects of motor speed. It may be mentioned that the writer happens to be at about the limit of practice in both the tapping test and simple reaction time. In simple reaction time (to sound and light) he has always been quite rapid; in the tapping test relatively slow. If the different aspects of motor speed do not correlate with each other, we certainly cannot expect a single aspect to give us a general idea of the individual's motor speed. The tapping test happens to be the easiest of the motor speed tests to make with a fair degree of accuracy, and it is probably as fundamental a point of individual difference as any other psychological test. At present it measures ultimately no more than it measures immediately, and indicates "voluntary motor ability" only in a very Pickwickian sense. Nevertheless the bare statistics of previous and the present experiments would seem to give assurance that there are conditions, almost certainly nervous, with which performance in this test varies from day to day and from individual to individual considerably beyond the limits of probable error. As these conditions are better made out, the gross rate will assume greater significance. At present, it is

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I the ratio of the pressed down period to the released period was about the same in right and left hands, and an immediate effect of practice was to decrease it very markedly, and about equally for both hands; that is, so far as can be judged by inspection, the key appears to be held down for a proportionately shorter period of the entire tap after practice than before. In Subject II it was also quite noticeable that the holding-down period decreased with practice in the right hand, and that it was from the beginning shorter in the right than in the left. Practice tended to make this difference more marked; the holding-down period decreased much more in the right hand than in the left. Indeed, one can hardly be sure that it decreases in the left hand at all. The length of this holding-down period varies a good deal from day to day, but as between the records of the same day, it becomes much more constant with practice. The length of this holding-down period may perhaps be taken to represent in a measure the efficiency of co-ordination, for the hand should obviously come up as soon as it touches the key; the direction of the hand's movement must be changed with as little loss of time as possible. The effect of practice is to shorten this interval, but above all to make it more constant, which latter is perhaps the truer attribute of efficiency in co-ordination. It may be mentioned that Binet and Vaschide with the myograph method found the pause between contraction and relaxation (corresponding to the holding-down period) to be longer than that between relaxation and contraction, though this result is incommensurable with the present experiments, since both movements here involve muscular contractions. With certain obvious modifications in method, the test might afford very interesting data in the improvement of rapid co-ordinations by practice.

impossible not to regard the changes in rate within the individual performance as the more important factors. Its technical advantages as a measure of fluctuation in continued work have been previously discussed.

2. *Index of right-handedness.* Woodworth has proposed an "index of right-handedness" based on the ratio of the efficiency of the left hand to that of the right. Our criterion of efficiency in this case is the average number of taps executed during five series of 30 seconds each. Dividing the average for the left hand by the average for the right, we obtain the "index." These indices for each of the ten subjects in the two experiments are as follows:

TABLE II

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
First experiment (right hand preceding.)	.93	.81	.87	.87	.90	.93	.94	.91	.90	.93	**.89**	*.035*
Second experiment (left hand preceding.)	.93	.87	.96	.87	.93	.85	.87	.93	.93	.93	**.91**	*.033*

In the average the left hand does relatively better when it precedes than when it follows the right, which would be the case if the work with one hand had a fatiguing effect upon the speed of the other, but in the individual cases this is seen to be the product of certain coarse deviations in either direction, so that no general conclusion ought to be drawn. For the subjects who do remain fairly constant, however, the individual differences in the index are worth noting.

3. *Rate in the successive series, "warming up."* We may now consider the data of Table I according to the heavy-faced figures in the vertical columns, the averages of the total efficiency for each 30" series taken by itself. In the right hand it is quite noticeable that the first two series are generally the poorest, and that there is a well-marked tendency for the later series to be faster than the earlier. To this phenomenon we may apply the name of *interserial warming up*, *i. e.*, a warming up from series to series, as distinct from a warming up process confined to a single series. The only trace of this latter process in normal individuals seems to be a slight increase in rate during the first second. This interserial warming up really appears

much more strikingly in the examination of the records of the individual subjects than it does in the averages, where it is entirely masked by the size of the m. v.'s. The tendency to progressive improvement can easily be traced throughout the right hand.

In the left hand it is by no means so evident that such a process exists. In the first experiment the five series average about equal in rate, and are very close together. In the second they are also quite close, the first series this time averaging the best. Some records seem to warm up, others to fatigue between the series, but on the whole the order is rather chaotic. The m. v.'s of these averages are an expression of individual difference between the subjects. It will be noted that these variations were distinctly greater in the second test than the first. This might possibly be taken to indicate that interest in the test was more nearly equal in the first than in the second experiment. Conscientiously co-operative individuals will work equally well up to their maximum at both times; on the other hand those subjects to whom novelty contributes the chief interest in the test, will co-operate better the first time than the second. Individual differences in the hands are practically equal, though they are slightly less in the left, not enough so for any special interpretation.

4. *Rate in the successive intervals, fatigue.* Let us now consider the data as presented from a different viewpoint, *i. e.*, according to the rate in each of the six successive 5″ intervals. (See Table III.) Since each series provides one of each interval, and the series are five in number to the record, each figure in the subjoined table gives the average number of taps during five cases of each five second interval. The precise way in which the figures are calculated may be understood by referring to the type record (p. 438). What 40.6, 38.2, 36.6, 35.8, 35.0, 34.2, are to the subject in the type record, 41.2, 38.8, 35.8, 35.0, 33.8, and 34.2 are to Subject A's first record with the right hand. During the first five seconds of each of his five series he averages 41.2 taps, during the second five seconds 38.8 taps, during the last five seconds 34.2 taps. As above, the figures are quoted separately for each of the two experiments. The average rates of the ten subjects for each interval as well as their m. v.'s will be found in their usual places.

Through these figures we may obtain an idea of the progressive decrease in efficiency which we term the fatigue loss. It appears that, so far as can be gathered from discrete units like the present, the curve follows the form usual to fatigue curves, the decrease being rapid at first and subsequently slower. The method is not such as to make it worth while to undertake measurements of very fine fluctuations; some

The figures are as follows :

TABLE III

RATES IN THE SUCCESSIVE INTERVALS IN TEN NORMAL INDIVIDUALS

AVERAGE NUMBER OF TAPS IN 5 CASES OF EACH INTERVAL

First Experiment, Right Hand

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
1st interval	41.2	27.6	32.8	36.2	36.4	37.2	32.0	38.6	39.0	32.4	**35.3**	*3.3*
2nd "	38.8	27.0	31.0	34.6	33.8	34.2	31.6	36.6	35.8	31.6	**33.5**	*2.6*
3rd "	35.8	26.0	31.0	33.8	31.8	33.6	31.2	35.6	34.4	30.6	**32.3**	*2.3*
4th "	35.0	25.2	29.6	33.2	30.0	33.6	30.8	34.4	33.6	30.2	**31.5**	*2.4*
5th "	33.8	24.8	29.4	32.6	29.8	32.4	31.0	33.4	33.0	29.6	**31.0**	*2.0*
6th "	34.2	24.6	29.6	32.6	29.4	31.4	30.4	33.0	32.2	29.2	**30.7**	*2.0*

TABLE III. (Cont.)

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
					Second Experiment, Right Hand							
1st interval	43.8	28.0	27.0	35.8	39.0	36.0	34.2	37.8	37.4	30.6	**35.0**	*3.9*
2nd "	40.0	26.6	27.2	34.6	35.4	34.4	33.0	36.0	35.0	30.2	**33.2**	*3.1*
3rd "	37.6	24.8	26.4	34.0	33.0	33.8	33.2	35.4	34.0	29.6	**32.2**	*3.2*
4th "	36.8	23.8	27.0	33.4	31.4	33.6	32.8	34.8	32.6	29.4	**31.6**	*2.9*
5th "	37.0	24.0	26.6	33.4	31.2	34.2	31.8	33.8	32.2	29.4	**31.4**	*2.9*
6th "	36.0	23.4	26.4	32.6	30.6	33.8	31.6	32.8	31.4	28.8	**30.7**	*2.8*
					First Experiment, Left Hand							
1st interval	40.8	23.6	27.6	32.2	30.2	32.0	32.4	35.8	34.2	31.2	**32.0**	*3.0*
2nd "	37.0	22.0	27.2	30.6	29.2	29.6	30.2	33.8	32.4	29.4	**31.1**	*2.9*
3rd "	33.6	20.2	26.6	29.0	29.4	28.0	29.8	31.8	30.8	28.2	**28.7**	*2.4*
4th "	30.6	20.4	26.0	28.6	28.2	27.0	28.4	31.6	29.8	27.8	**28.0**	*2.1*
5th "	30.8	20.4	25.6	28.2	27.8	26.2	28.0	29.6	29.6	26.8	**27.3**	*2.2*
6th "	29.2	20.0	25.6	27.4	27.8	25.4	27.2	30.0	29.6	25.6	**26.9**	*1.9*

TABLE III. (Cont.)

Second Experiment, Left Hand

Subject	A	B	C	D	E	F	G	H	J	K	Av.	M. V.
1st interval	42.8	24.4	26.6	32.4	34.0	32.0	30.4	35.2	35.0	29.8	**32.3**	*3.5*
2d "	38.2	23.4	26.0	30.4	33.0	30.6	30.2	34.8	32.6	28.6	**30.8**	*3.2*
3rd "	34.6	21.6	26.0	29.8	31.2	28.8	29.0	32.4	31.6	27.8	**29.3**	*2.6*
4th "	33.6	21.0	25.8	29.0	30.2	28.0	28.2	30.2	31.0	27.0	**28.3**	*2.4*
5th "	32.4	21.0	25.6	28.2	29.6	27.8	27.0	32.2	30.2	26.6	**28.1**	*2.3*
6th "	32.4	20.8	25.2	28.4	29.4	27.0	26.0	30.2	29.4	26.2	**27.5**	*2.3*

rather limited researches upon this point have already been mentioned.[1] Great individual differences in the regularity of the single taps can be seen, however, by inspection, and this is an aspect of the test that would well repay special study, though it is not within the scope of the present calculations. It may also be brought to mind that as the figures given are averages, the decreases which they represent are frequently of greater regularity than characterizes the scores for the individual series, the irregularities tending to compensate; but the averages also compensate for the error introduced by taking no account of fractions of a tap, which, when the rates are very constantly maintained, is often of no inconsiderable significance.[2]

5. *Fatigue and individual variation.* So far as gross rates are concerned, the figures of course merely restate the data given in Table I; but the m. v.'s seem to present unequivocally a point of some significance. It will be noted that they throughout tend to become progressively smaller as fatigue sets in, quite out of proportion to the decrease in gross rate. This is true of all the records.

The individual differences in the function are decreased by from 1/4 to 1/3 after 30 seconds work. *The main factor then, in giving a high gross rate to an individual at the beginning of practice is his performance in the earlier intervals.* When we are

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[1] Bliss: Yale Studies, I, 45-52; Moore: Yale Studies III, 92-95.

[2] There must be mentioned in this connection a series of eight experiments with an additional mentally normal subject, with no known neurological condition, who fails entirely to show any of the principal phenomena of the tapping test indicated as normal in these and previous studies. Of co-operation, at least to the level of that reached in any of the ten normal subjects, there can be no question. The results are marked by very wide fluctuations between the intervals as well as from series to series, and even from day to day. Of fatigue phenomena, in the sense in which the term has been here used, there is no significant trace, but as has been said, there are very wide, almost chaotic fluctuations, between the intervals. Examples are such series as the following:

a.	*38*	*40*	*35*	*39*	*35*	*36*
b.	*40*	*38*	*37*	*40*	*39*	*38*
c.	*34*	*33*	*35*	*40*	*40*	*38*
d.	*36*	*36*	*36*	*36*	*38*	*36*
e.	*30*	*31*	*32*	*31*	*36*	*32*

Now and then occurs a series whose intervals resemble the familiar fatigue curve, as 34, 33, 31, 29, 29, 29, but this no oftener than might be the product of chance. Indeed, if there is any general tendency at all it is for the later intervals to be faster than the earlier, *i. e.*, they show intra-serial warming up, warming up within the series. During the eight consecutive days there is no trace of practice; indeed, the later experiments are rather slower than the earlier. The writer cannot formulate any explanation of these wholly anomalous records.

fresh, and doing the best that we shall do, we vary distinctly more from each other than when we have lost somewhat through fatigue. The nearest interpretation might again seem to be in terms of co-operativeness. The more co-operative individuals tried harder, and fatigued more quickly to the level of those who did not try so hard. In this sense however, we have to interpret co-operativeness, not in terms of consciousness, but rather as the amount of effort which the organism puts forth in response to volition. It may be said with some assurance that continued exercise of the function, besides lowering its efficiency, tends also to decrease individual differences in it.

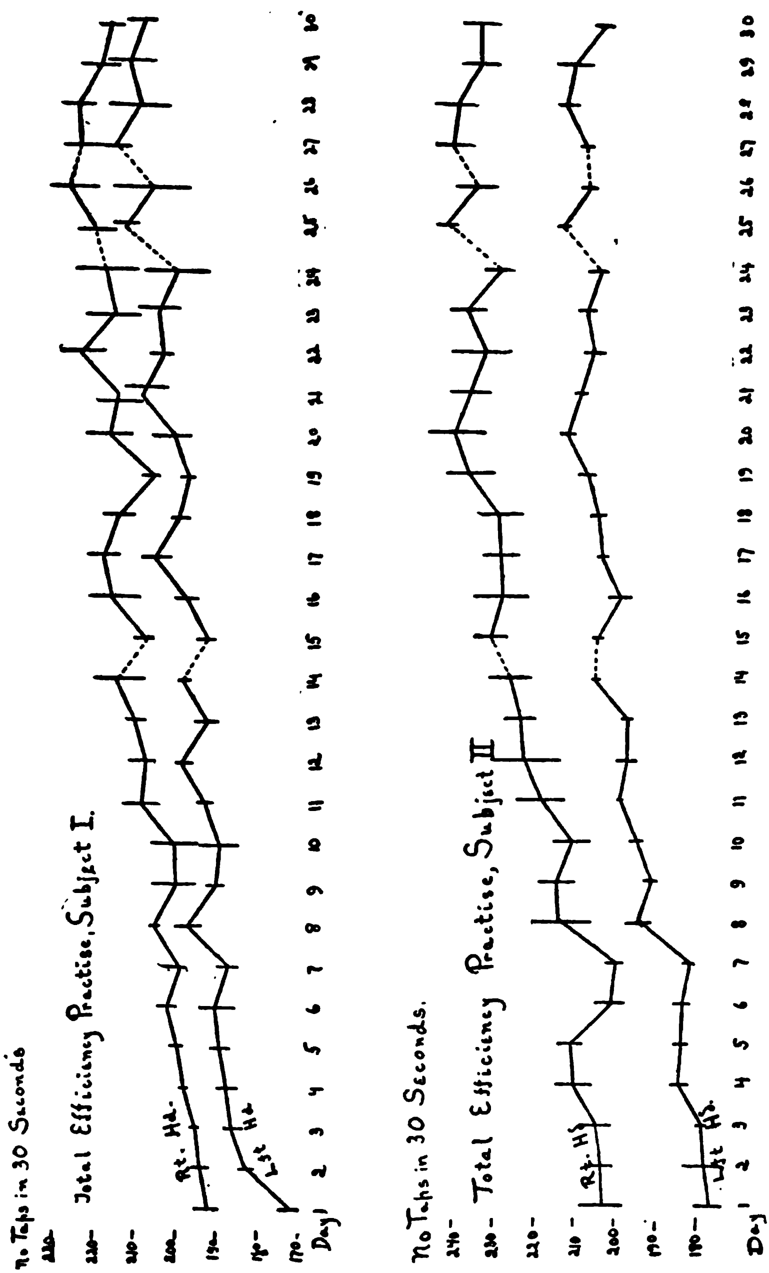
6. *Total efficiency practice for the two subjects.* Such are the general phenomena of the tapping test as they appear in ten normal individuals without special practice. We shall now observe how these conditions are modified during practice in two normal individuals. As has been said, the series on Subject I is continuous except for intentional breaks in practice, but the first three experiments on Subject II were separated by considerable intervals which do not, however, appear to affect their results. In each subject intermissions of 14 days were introduced after the 14th and of 10 days after the 24th and 26th days; this being done to study the effect of such intermissions on the practice curve. The entire curve is made up of 30 experiments.

As a basis for plotting the general course of the practice curve in the two subjects, the natural choice fell upon the "total efficiency" figure for each record, corresponding to **220.4** in the type record. The accompanying plate shows the practice curve as given for the successive thirty "total efficiency" figures. The dotted lines, here and in Plates II and III, mark the intermissions. The vertical lines give the m. v. of each total efficiency figure.

It should perhaps be mentioned that Subject I had had some relatively unsystematic practice in the test, both as a subject in other researches of which it formed a part, and in setting up the present apparatus. Subject II was quite without special practice. The curves, however, show no characteristic that could be referred to this difference.

First of all, it will be noted that the rise ot the curve is not more rapid at the beginning than elsewhere, the opposite of what we usually expect in practice curves. This is especially marked in Subject II, in whom we should expect it least, as he was without special practice; there is even a distinct drop in efficiency before a significant practice gain begins. This would seem to indicate that such unsystematic practice in this function as we receive in normal life largely eliminated the

PLATE I


No Taps in 30 Seconds
Total Efficiency Practise, Subject I.
Rt. Hd.
Lft Hd.
Day
No Taps in 30 Seconds.
Total Efficiency Practise, Subject II
Rt. Hd.
Lft Hd.
Day

2

marked gains so frequently seen at the beginning of practice curves. Strictly speaking, there is of course no function in which we can get the real beginning of a practice curve, any more than we can ever be really certain of having reached its end. The last ten experiments on both subjects show no especial practice gain, and so far as they go indicate the limit of practice to have been reached; but in a series of later experiments, not reported here, separated from these by an interval of nearly a month, a distinctly higher rate is obtained with Subject I. The same might or might not be true of Subject II.

7. *Relative practice of right and left hands.* Again, in neither subject does the left hand show an improvement relative to the right. In Subject I the index of right-handedness remains practically the same. In Subject II the right hand may even improve more than the left. So far as this function is concerned then, the asymmetry does not seem to be a matter of practice, at least during adult development. If it is such during earlier years, and there are so far as I know no data on this point, it might indicate that the organism is no longer sufficiently plastic to respond differently according to differences in previous training, yet it is difficult to see how under such conditions the hands would respond to practice at all, as they certainly do. At the present time the superiority of the right hand in this function seems permanently "set;" whether it was formerly otherwise is not a question which these data can answer.

In Subject I the left hand curve for the first twenty experiments follows the right very closely from day to day, rising and falling with it in every case but two (12th and 13th days), often in much the same ratio. This would indicate that the conditions bringing about a rise or fall in rate were of a general nature, since they tended to affect both hands similarly. On the 21st day, however, precisely the reverse condition begins, and is maintained to the end of the experiments save for the 30th day. The indices, as is evident below, show that the approach to this condition was gradual. When the right efficiency increases, the left decreases, and *vice versa*. It will be remembered that the precedence of the two hands alternates from day to day. Under these later conditions each hand does relatively better when it *follows* the other hand than when it precedes. It is as though the work done with the preceding hand effected a generalized warming up, contributing to the greater efficiency of the following hand.

The record of Subject II shows the same general correspondences, though with rather more frequent exceptions, seven in number. There is no evidence of the generalized warming-up

at the end, as in Subject I. On the whole, then, the tapping rate seems to be the expression of a general condition.

It will be noted that Subject I's performances for the first seven days vary very little among themselves, the curve being almost a straight line, slightly ascending. At this point we begin to encounter relatively wide fluctuations from day to day, which continue throughout the experiments. In Subject II this is also seen in the left hand; not so well in the right. Such an appearance would indicate that at the beginning a fairly constant limit was set to the maximum rate, which was later superseded by another limit of a much more variable character. The first limit may be one of co-ordination; the second, one of the actual discharge rate of the nervous impulses, but this is purely hypothetical.

From these data it follows that the index of right-handedness should not be subject to any marked increase or decrease as an effect of practice. The indices in each of the thirty experiments for both subjects are given on Table IV, p. 456.

In Subject I the average does not essentially change, and the low index in the first experiment is probably accidental. Nevertheless there is another point of interest. It may be noted that beginning with the 16th experiment the size of the index alternates quite regularly from high to low, although this is not strikingly apparent in the practice curves till the 21st day. The high index, *i. e.*, the relatively greater efficiency of the left hand occurs when the right hand precedes the left in the experiment. This is not shown by Subject II, whose indices are very constant from day to day, and indeed throughout, though they seem to rise slightly during the second five days.

The hands are very much closer together in Subject I than in II. The right hand of I does not improve in practice so much as that of II, nor does the left, if we do not consider I's first day. The difference is, however, very small, and in relation to the gross rates quite negligible.

It is safe to say that the intermissions, indicated by dotted lines, could not be located unless they had been so indicated. But while the intermissions do not essentially affect the gross rate as given in the figures here presented, they do affect the performance considerably, as more detailed considerations will subsequently show.

The m. v. of each day's series among themselves tends to increase as shown by the increase in the length of the vertical lines. Practice is ordinarily supposed to bring m. v.'s down, and it very probably does decrease that of the individual taps for short periods, but as between series and series, it introduces a warming up effect that increases their m. v. To what ex-

TABLE IV

Exp. (Day)	1	2	3	4	5	6	7	8	9	10
Subject I.	.90	.94	.96	.95	.95	.94	.93	.96	.95	.94
Subject II.	.87	.87	.87	.88	.87	.91	.91	.91	.89	.92
Exp. (Day)	11	12	13	14	15	16	17	18	19	20
Subject I.	.92	.96	.92	.92	.93	.92	.95	.93	.96	.92
Subject II.	.91	.88	.88	.91	.88	.87	.90	.89	.88	.89
(Exp. Day)	21	22	23	24	25	26	27	28	29	30
Subject I.	.97	.91	.95	.92	.97	.91	.96	.93	.97	.96
Subject II.	.88	.89	.87	.87	.88	.88	.86	.89	.90	.87

tent the increased m. v. is due to this factor may be gathered by comparing the other three curves to that of Subject II's

left hand. The warming up increase is practically absent here as is also the progressive increase in the m. v.

8. *The effect of practice on the "warming up" phenomenon.* So much for the general effect of practice on gross efficiency. In further analysis we must determine the special way in which this increase manifests itself, whether it occurs through a general increase in efficiency throughout the fatigue curve, or is mainly a heightening of the initial rate, or the giving of a progressive immunity to fatigue. Also whether the increase manifests itself differently in different series, the earlier series of an experiment not gaining so much as the later, or what not. This is the point that we shall consider first, and the data bearing upon it are given in Table V, pp. 458-9.

In this table the thirty experiments are divided into three groups of ten each, the figures in the vertical columns giving the average and m. v. of each series from day to day for each set of ten. There will at once be noted a perfectly definite tendency for the later series in each experiment to be faster than the earlier, that is, the efficiency of the hand warms up during the experiment. Inasmuch as the last series is almost always the fastest, we cannot say from these experiments how far the warming up process would extend, or how much further increase there might be.

When we observe this warming up process as it appears in the successive groups of ten experiments, it is evident that the warming up, (*i. e.*, the balance in favor of warming up over persistent fatigue) is very much accentuated by practice. In the ten normal individuals we could observe its presence in the right hand even without special practice, but by no means so distinctly as it appears in the tables here. In Subject I there was really no trace of the warming up until the fourth or fifth experiment, though in the right hand of Subject II it was quite evident from the start. The increase tends to become progressively greater with each group of ten, and the phenomenon is much more clear in the individual records than the m. v.'s of the averages would indicate it to be. The day to day variation in gross rate brings up the m. v.'s without really affecting the relation in which the different series of the same day stand to one another. This can be seen in another way. It will be noted that while the difference between the successive series of the same day (as given in the m. v., Plate I, vertical lines) tends to increase with practice, the m. v. of the same series on successive days (Table V) tends to decrease with practice; the day to day performance in each series tends more and more to form a species within itself.

The influence of practice in bringing out the warming up process could not be fully shown without printing the entire

set of series in full, which is hardly worth while; nevertheless some further demonstration of the fact may be found in the

TABLE V

Average number of Taps in each of the five successive 30″ Series, for the first 10, the second 10, and the third 10 days of the thirty experiments. The M. V. gives the day to day variations of each series.

Subject I

	Rt. Hd.						Lft. Hd.					
Days	1–10.		11–20.		21–30.		1–10.		11–20.		21–30.	
	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
1st series	194.6	3.6	204.3	3.5	209.5	4.1	184.6	3.7	194.3	2.6	197.3	5.1
2nd "	195.4	4.4	206.5	3.1	213.7	3.7	183.8	4.7	192.9	2.9	202.3	3.8
3rd "	197.7	3.9	212.7	4.3	221.5	3.7	188.4	6.2	198.9	3.7	207.7	3.1
4th "	200.8	3.1	215.4	4.8	223.6	2.2	188.9	4.5	199.5	4.0	211.9	5.5
5th "	201.1	5.5	218.6	4.5	224.9	3.7	189.1	4.7	200.2	3.4	212.7	3.1
Average	197.9	4.1	211.5	4.0	218.6	3.5	187.0	4.8	197.2	3.3	206.4	4.1

TABLE V. (Cont.)

Subject II

	Rt. Hd.						Lft. Hd.					
Days	1–10.		11–20		21–30		1–10		11–20		21–30	
	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
1st series	201.2	3.8	217.2	4.4	227.0	2.4	187.2	4.4	203.8	4.8	209.1	2.6
2nd "	203.5	4.5	224.6	6.0	232.9	3.9	182.2	4.6	200.7	4.5	206.3	3.9
3rd "	207.5	5.5	230.3	5.3	237.0	3.7	184.8	6.0	203.6	4.4	210.0	4.3
4th "	210.9	6.5	234.2	4.4	240.7	3.5	183.5	6.4	203.4	3.8	207.7	2.7
5th "	213.9	5.1	234.6	5.8	242.4	3.6	184.9	5.9	203.5	3.5	206.4	2.8
Average	207.4	5.1	228.2	5.2	236.0	3.4	184.5	5.5	202.9	4.2	207.9	3.3

following curves, which represent the right hand practice through the thirty days for the first and last series of each day

PLATE II

Practice curves of the 1st and 5th series of the right hand in each subject. The warming up from the 1st to the 5th series, and its accentuation by practise, are shown by the curve for the 5th series drawing away from that of the 1st series.

(corresponding to 213 and 227 in the type record), just as the practice curves previously quoted gave the average of all five series. As before, the dotted lines mark the pauses. It will be noted that the curve for the last series draws away from that of the first, showing its more rapid increase in rate. The practice gain in Subject I and in the right hand of Subject II are thus seen to consist largely in the increase in rate of the later series over the ealier.

In the left hand of Subject II this warming up is not in evidence, but this does not mean that it is absent. We must always bear in mind that we never study fatigue alone, or warming up alone, but the resultant of the two, and it is according as one overbalances the other that we speak of the work as showing "warming up" or "fatigue." It is perfectly possible that the conditions of warming up are potentially present but are overbalanced by fatigue conditions; so far as sensations go this would be borne out by the introspection of the subject, who never entirely lost a progressive sensation of fatigue in the left hand record. In Subject I the left hand showed no more tendency to fatigue sensation than the right after the first few days of practice. It will be remembered that also in the ten subjects the warming up was much more marked in the right hand than in the left. This would be in accord with the result of practice as bringing out warming up, since in these subjects also the right hand was presumably more practiced for this function than the left.

9. *Practice and the successive intervals.* Now given the result that the practice increase of the "total efficiency" figure comes in as an increase of the later series rather than of the earlier, what is the character of the gain in the intervals of the individual series itself? Is it an approximately equal gain for all intervals, is it an increase in initial efficiency or is it an increase mainly in the later intervals through growing immunity to fatigue?

With reference to this point we must consider the data in the same way as they were presented for the ten normal subjects in Table III, *i. e.*, interval by interval. The experiments have again been divided into 3 groups of 10, the averages and m. v.'s for each successive interval being given in Table VI, pp. 462-3.

10. *The influence of intermissions upon practice gain.* The most interesting aspect of practice phenomena, however, is afforded in a study of the effect of intermissions. The days after the intermissions are, as noted, the 15th, 25th, and 27th days. Referring to Plate I we see that in total efficiency these days show for Subject I a uniform loss with the right hand; with the left, however, there is a loss only with the first intermission,

the other two showing considerable gains. In the curves given in Plate III, p. 471, we can analyze these gains somewhat.

TABLE VI

Number of Taps in each of the successive 5" intervals. Average of 50 cases of each interval, 5 for each of ten days. The M. V. gives the day to day variation of the averages for the 5 cases of each interval for each day.

Subject I

	Rt. Hd.						Lft. Hd.					
Days	1–10		11–20		21–30		1–10		11–20		21–30	
	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.	Av.	M. V.
1st interval	36.4	1.04	39.2	.80	40.8	1.06	34.2	1.06	36.1	.94	38.1	.76
2nd "	34.7	.52	37.2	.62	38.8	.50	32.7	.79	34.6	.50	36.1	.69
3rd "	33.3	.60	35.6	.56	36.5	.62	31.2	.72	33.1	.46	34.6	.54
4th "	32.1	.59	34.0	.72	35.1	.48	30.4	.64	31.7	.60	33.3	.50
5th "	31.1	.54	33.0	.82	33.9	.51	29.5	.78	31.2	.60	32.4	.56
6th "	30.6	.82	32.3	.78	33.4	.52	28.9	.46	30.4	.44	32.0	.58

TABLE VI. (Cont.)

Subject II

	Rt. Hd.						Lft. Hd.					
Days	1–10		11–20		21–30		1–10		11–20		21–31	
	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.	Av.	M.V.
1st Interval	37.2	.88	41.2	.92	42.1	.72	33.7	.75	36.5	.90	37.1	.72
2nd "	35.8	.86	39.5	.88	40.7	.62	31.9	.80	35.4	.56	36.2	.46
3rd "	34.7	.94	38.0	.78	39.4	.40	30.7	.96	34.2	.84	35.2	.42
4th "	33.7	.88	37.0	.84	38.4	.44	30.0	.92	33.2	.58	34.0	.44
5th "	33.1	.80	36.4	.80	37.8	.62	29.4	.92	32.3	.56	33.2	.42
6th "	32.6	.68	36.1	1.00	37.5	.78	28.8	1.06	31.7	.64	32.2	.44

The principal point to be noted in this table is that the practice gain is in Subject I more prominent in the earlier intervals; increase therefore is mainly one of initial efficiency. In Subject II the intervals tend to gain about equally.

The most striking feature about them is that in both subjects the initial interval invariably makes a considerable gain. In-

deed, so far as the initial efficiency is concerned, one could hardly be sure that there would have been greater gain if the practice had been continued throughout the intermission. On the other hand, it is apparent that in Subject I there is much greater susceptibility to fatigue immediately after the intermission; on the 15th and 27th days all the later intervals lose, and on the 25th day the gain of the later intervals is by no means proportionate to that of the first. Subject II on the other hand, shows a very marked gain throughout all the intervals on the 15th, 25th, and on the 27th days, in every case but one, the fifth interval for the 15th day. Only on the 15th day is any special susceptibility to fatigue indicated, though it is true that the fatigue loss is on each of the three days greater than the average for the group of ten experiments in which each belongs. Both subjects found the sensations of fatigue to be much more marked on the day after an intermission, and indeed these were in Subject I practically the only times when they appeared at all. The great initial gain after the intermissions is by no means easy of explanation. It seems rather forced to interpret it as the result of a renewed "*Neuigkeitsantrieb*," especially for Subject I. It seems to be a fairly general observation, however, that partially practiced co-ordination paths may subsequently become more firmly established during a period of rest ("learning to swim in winter and to skate in summer," etc.). The intermission gain here noted is probably an aspect of this, presumably a physiological phenomenon.

11. *The effect of warming up, as brought out by practice, on the different intervals.* In order to gain some idea of the extent of the warming-up process, it was made the subject of four special experiments, which are not included in the curves. These succeeded immediately upon the twenty-fourth day of practice, and consist each of ten series made with one hand alone. The first and third of these experiments were made with the right hand, the second and fourth with the left, the routine being otherwise the same as in the regular experiments. Inasmuch as these experiments show fairly well the behavior of each hand at a high degree of practice, and illustrate many points in it that would otherwise be impossible to bring out concretely, a right and left hand experiment for each subject is reproduced in full.

These records show immediately that in the right hand at least, the warming-up process is by no means exhausted in the first five series; on the contrary, the second five series are uniformly better than the first. The individual series improve in rate up to a certain point, and then the warming-up process is no longer sufficient to counterbalance the unrecovered fatigue from series to series, when the series begin to fall off. The

TABLE VII

Experiments of 10 series each upon the same hand throughout of Subjects I and II, after 24 days of practice. The series followed each other at 3′ intervals.

	Subject I. (Rt. Hd.)								Subject II. (Rt. Hd.)						
	1st interval 0″–5″	2nd " 5″–10″	3rd " 10″–15″	4th " 15″–20″	5th " 20″–25″	6th " 25″–30″	Total.		1st interval 0″–5″	2nd " 5″–10″	3rd " 10″–15″	4th " 15″–20″	5th " 20″–25″	6th " 25″–30″	Total.
1st series	40	37	35	34	32	32	210	1st series	41	38	36	36	35	35	221
2nd "	38	38	36	38	38	32	210	2nd "	40	39	38	38	37	36	228
3rd "	40	39	36	35	34	33	217	3rd "	41	40	40	39	38	37	235
4th "	41	39	38	37	35	36	226	4th "	42	41	40	40	40	39	242
5th "	42	39	38	38	36	35	228	5th "	42	42	40	40	38	38	240
6th "	41	40	37	36	36	35	225	6th "	43	43	40	40	40	40	246
7th "	42	41	38	38	36	36	231	7th "	43	42	42	41	41	40	249
8th "	40	41	39	37	35	35	227	8th "	43	42	41	40	41	40	247
9th "	41	39	37	36	35	34	222	9th "	43	42	42	40	40	39	246
10th "	40	38	37	35	34	35	219	10th "	43	41	40	39	39	40	242
Av.	40.5	39.1	37.1	35.9	34.6	34.3	221.5	Av.	42.1	41.0	39.9	39.3	38.9	38.4	239.6

TABLE VII. (Cont.)

Subject I. (Lft. Hd.)	1st interval 0″–5″	2nd " 5″–10″	3rd " 10″–15″	4th " 15″–20″	5th " 20″–25″	6th " 25″–30″	Total.
1st series	39	35	33	33	30	31	201
2nd "	39	35	34	32	32	32	204
3rd "	40	36	35	34	33	32	210
4th "	38	37	35	35	33	33	211
5th "	40	38	36	35	32	34	215
6th "	39	38	35	35	33	32	212
7th "	38	36	34	33	33	31	205
8th "	38	37	35	33	33	32	208
9th "	35	35	32	33	32	31	198
10th "	36	35	34	33	33	31	202
Av.	38.2	36.2	34.3	33.6	32.4	31.9	**206.6**

Subject II. (Lft. Hd.)	1st interval 0″–5″	2nd " 5″–10″	3rd " 10″–15″	4th " 15″–20″	5th " 20″–25″	6th " 25″–30″	Total
1st series	36	36	34	32	33	32	203
2nd "	35	36	34	33	33	33	204
3rd "	38	37	37	35	35	34	216
4th "	38	37	36	35	34	33	213
5th "	39	37	36	35	34	33	214
6th "	38	38	36	34	34	32	212
7th "	37	37	35	34	34	31	208
8th "	37	37	36	35	33	33	211
9th "	37	37	34	33	32	31	204
10th "	37	37	35	35	33	31	208
Av.	37.2	36.9	35.3	34.1	33.5	32.3	**209.3**

maximum in the right hand is never reached until the seventh or eighth series. In the left, on the other hand, the maximum is reached more quickly, and we see more of the falling off process; this much more clearly in Subject I. The second five series with the right hand are distinctly better than the first five, though in the left hand the reverse is true in the series quoted.

In further examining the records, especially those of the right hand, in which the phenomenon is more clear cut, we see again the point already brought out regarding the more precise character of this warming up gain. It is a gain that affects all the intervals somewhat, but the later rather more than the earlier; *i. e.* the gain comes mainly through an increased immunity to fatigue. Thus we see that while the initial performance of Subject I's right hand increases only from *40* to *42*, the final interval increases from *32* to *36*. In the left hand the initial gain is practically *nil;* that of the final interval from *31* to *34*. The initial interval of Subject II's right hand gains from *41* to *43*, the final interval from *35* to *40*. His left hand, as has been noted, is anomalous with respect to the warming up phenomenon. Here we find the initial intervals gaining, while the final ones tend to lose. This loss is accompanied by considerable sensation of fatigue.

This warming up increase in the later intervals probably persists a considerable time; longer at least than the fatigue that we see gradually lowering the efficiency of the later series. This appeared in a group of subsequent experiments on the right hand of Subject I in which two records of five series each were made within one hour 45 minutes of each other. Table VIII, p. 468, will illustrate this point:

The final intervals in the 3:45 record warm up from **31** to **35**, with a considerable increase also in the initial interval; those of the 5:30 record begin at **34** and warm up to **37**, with negligible increase in the initial interval. Sometimes, also, the final interval does not warm up at all in the 5:30 record; but the essential point is that the final interval in the 5:30 record always begins considerably ahead of that in the 3:45 record. It should be mentioned that series taken at 8:45 A. M., show in all respects the same characteristics as those of 3:45 P. M. on the same day.

12. *An "index of fatigue."* It is of course impossible to obtain a complete view of the fatigue phenomena in any experiment in the absence of the entire curve. But while the record does give us a curve of this sort, it is quite impracticable to evaluate all its factors, and as has been said, we

TABLE VIII
Subject I, Right hand

	3:45 P. M.							5:30 P. M.						
	1st interval 0"-5"	2nd interval 5"-10"	3rd interval 10"-15"	4th interval 15"-20"	5th interval 20"-25"	6th interval 25"-30"	Total.	1st interval 0"-5"	2nd interval 5"-10"	3rd interval 10"-15"	4th interval 15"-20"	5th interval 20"-25"	6th interval 25"-30"	Total.
1st Series	40	37	36	33	33	31	210	42	40	37	36	35	34	224
2nd "	38	37	36	35	34	32	212	39	38	36	36	36	34	219
3rd "	42	39	37	37	35	34	224	42	40	37	37	35	36	227
4th "	42	40	39	36	36	34	227	42	42	39	37	36	36	232
5th "	43	40	39	37	37	35	231	43	40	38	38	37	37	233
Av.	41.0	38.6	37.4	35.6	35.0	33.2	220.8	41.6	40.0	37.4	36.8	35.8	35.4	227.0

consider merely the amount done in the successive 5" intervals. This together with the inspection of the record, will give a very fair idea of all but the finest fluctuations. And yet when we consider the number of individual curves with which we have to deal it will readily be seen that it is impossible to state the phenomena even at this length in their entirety. All things

taken into account, it is probably best that we should use some single figure as the expression of fatigability. This figure, which is here used as the "index of fatigue" and which will subsequently be denoted by *f*, is calculated by taking the average number of taps executed in the 2d, 3d, 4th, 5th and 6th intervals, and dividing it by the number of taps executed in the first interval. Thus, a curve may run as the first one in the type experiment on p. (438), *41, 37, 35, 34, 34, 32*. The number of taps in the first interval being *41* and the average number in the remaining five intervals being 34.4, *f* equals $\frac{34.4}{41.0}$ or .85.

This figure gives us the gross amount of decrease in rate during a certain period, though, of course no idea as to the form or speed with which the decrease sets in. Its main arithmetical source of error is that the number of taps made in the first five seconds is rather coarse on account of being given in the form of an integer, its probable error being about half a tap each way. *The higher the f the greater the immunity to fatigue; the lower the f the greater the susceptibility.*

13. *Correlation of the fatigue phenomena in the different hands.* All statements of individual differences, correlations, etc., in fatigability, are made upon the basis of this *f*. It is possible in a measnre to test the validity of the *f* by comparing the results with the right and left hands. The results ot the ten subjects show a slight tendency for the individual who is fatigable with his right hand to be also fatigable with his left, and for the day of greater fatigability with one hand to correspond to the day of greater fatigability with the other.

It has already appeared, however, in other aspects of the results, how much the delicacy and responsiveness of the test is increased by practice. The results of the practised subjects seem to indicate a greater correspondence between the fatigability of right and left hand than is found in the ten normal subjects, though even here the conclusion depends somewhat upon the method by which we treat the data. Taking the average *f* for the five series of each day as most representative of the *f* for that day, we have for the thirty experiments thirty *f*'s for each hand. Arranging the thirty *f*'s for the right hand in order of size, and comparing this order with that of the left hand, we find between them 37% of displacements for Subject I, 43% for Subject II.[1] This is again but

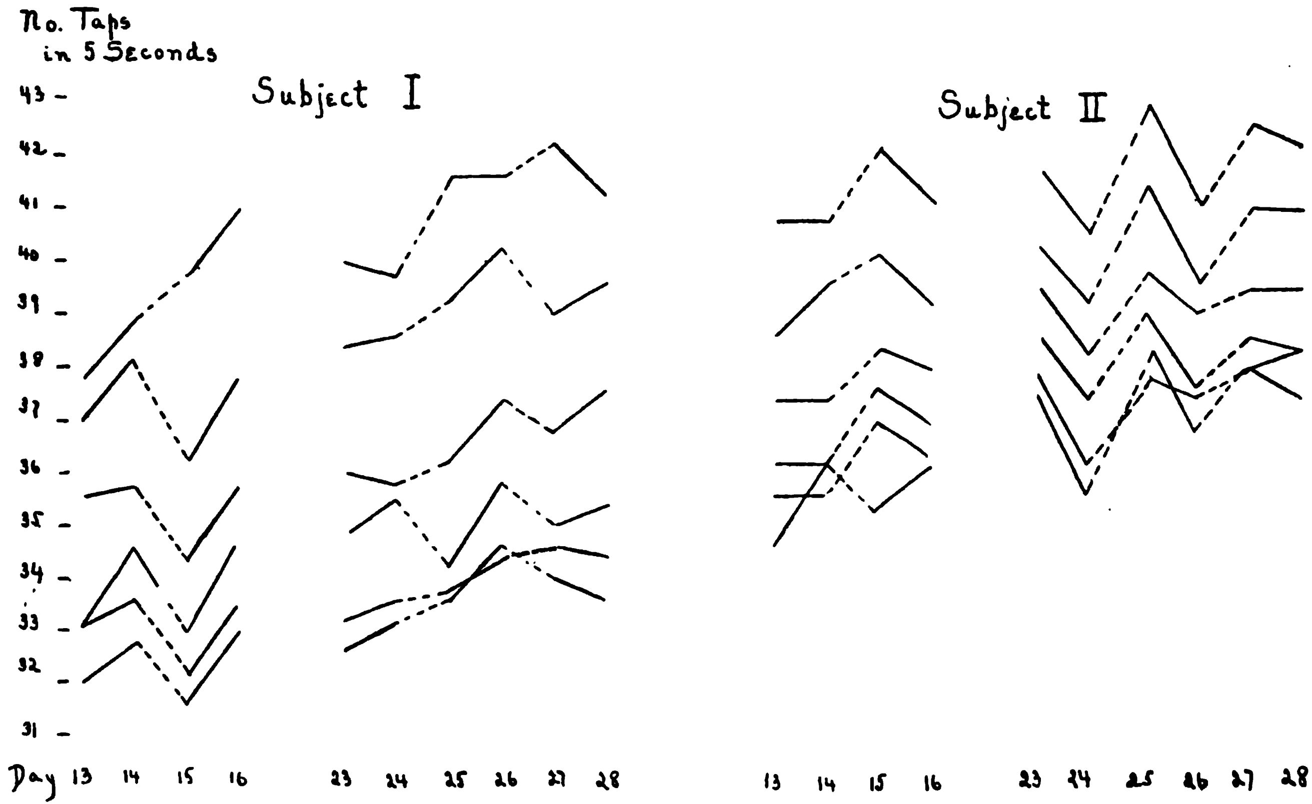
[1]With reference to this correlation method *cf.* Ruediger, The Field of Distinct Vision, *Arch of Pych.* 5, pp. 37-8; Wells, A Statistical Study of Literary Merit, *Arch of Pych.* 7, p. 23.

a small positive correlation. We may also observe whether the right and left hands tend to increase or decrease together from day to day; this seems to the writer perhaps the most logical way of treating data of this precise nature. We may plot curves of the f's of the right and left hands for different days, and superpose them. This gives us the figure in Plate IV. In examining these curves, we may note that with respect to ascent or descent the left hand follows the right with considerably more frequency than is provided for by chance. The chance correspondence would be 14.5, 50% of the cases; it actually occurs in 22, 76% of the cases for Subject I, and 21.5, 74% of the cases for Subject II, as can be seen by the study of the curves. It may be noted that the fewer negative and zero cases are more likely to be extreme than the more numerous positive ones, *cf.* days 15, 24 and 28 for Subject I, and 7 and 8 for Subject II. This would again suggest that the negative relationships were the product, not of chance variation, but of the intercurrence of uncontrollable conditions affecting one hand especially. To the extent to which positive correlation between right and left hand exists, the measure is one of a general condition; for the rest it refers only to the specific neuro-muscular mechanism involved in the movements of either hand.

In the face of these indisputable cases of the right hand showing one extreme, the left the other extreme of fatigability, and this in a test of such high mathematical precision as the present, it is plain that we must either postulate a considerable influence of the work of one hand upon that of the other or else largely give up the measure as an index of a general fatigability. In a few instances, especially in the later practice of Subject I, the former possibility would seem justified, but there are still marked cases as that of the 8th day in Subject II which cannot be explained in this way. While, then, the two hands do seem to have a tendency to correlate positively, each hand seems also to be subject to so many effects specific to itself—irrelevancies, as Spearman would call them—as largely to limit the value of the single measure as a general measure. And we cannot be sure that the fatigue phenomena of different functions, as the tapping test, the ergograph and the addition test, would exhibit a traceable correlation, at least in normal individuals.

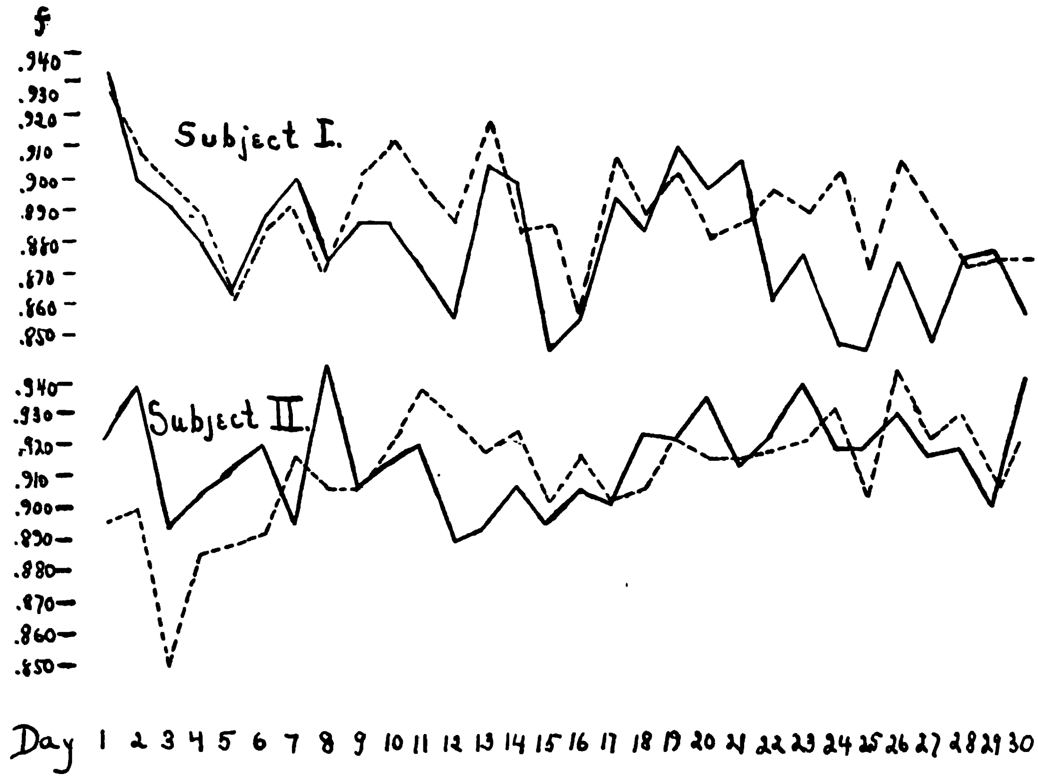
14. *The relative fatigability of right and left hands.* There seems to be no regularity in the relative fatigability of right and left hands among different individuals; in some the preferred, in others the unpreferred hand shows the greater immunity to fatigue. More frequently, however, we find that the preferred hand has the greater immunity to fatigue, and

PLATE III



Records of the right hand on the days immediately preceding and following intermissions, which are given in dotted lines. There is an unrecorded practise of 20 series during the intermission before the 25th day (unrecorded in the curves), succeeding immediately upon the 24th day. (*Cf.* p. 464.)

PLATE IV



The course of the f during practise. The continuous line gives the f of the right hand, the dotted line the f of the left hand, in both subjects. These curves show the general effect of practise on fatigability, as well as the relationship of the two hands.

this is the relationship which obtains in the averages for the ten normal subjects. The relationships in the two practiced subjects can be best seen by again referring to Plate IV. In Subject I the fatigability of the right and left hands is about equal at first, in Subject II the left hand is much more susceptible than the right; but it will be seen that the effect of practice is in both subjects to immunize the left hand to fatigue more than it immunizes the right.[1] In Subject II they end about equal. A possible interpretation of this fact is as follows: Practice diminishes fatigue sensations, and since the left hand is more subject to fatigue sensations than the right, it probably diminishes those of the left more than it does those of the right. If, then, fatigue sensations have an inhibiting effect on the tapping rate, the result is entirely what we should expect. But it must be remembered that in this experiment the relation between fatigue sensation and fatigue of performance is only an indirect one. In the ergograph it is the muscle which does the work, and the muscle which shows the fatigue sensations; but in the tapping test there is every reason to believe that the muscles would, during any fatigue conditions that are reached in the present experiments, execute an indefinite number of taps if only they received the innervations fast enough. The objective fatigue phenomena which we note in the test are in all probability either a fatigue phenomenon in the refractory phase or a lowered efficiency of co-ordination, equally a product of altered synaptic conditions; the sensations of fatigue on the other hand, may with equal assurance be ascribed to tissue changes within the muscles that take place as a result of their continued effort. In this test, therefore, the fatigue sensations are absolutely no indication of the actual fatigue conditions, and any traceable correspondence between fatigue sensation and fatigue of performance must be regarded as almost wholly a product of reflex inhibition. In this way we can perhaps judge something of the relative responsiveness of the two subjects to sensations of fatigue. As noted, sensations of fatigue are in both subjects especially prominent the day after an intermission, but Subject II shows no special objective susceptibility on such days, Subject I a great deal. By itself, this result could be taken to mean that Subject I was more responsive to sensations of muscular fatigue than Subject II. But, on the other hand, Subject I had practically no sensations of

[1]The question is actually more complicated than appears here, involving in Subject I certain questions of the relation of warming up, practice and fatigability; but the matter is rather too complex in proportion to its importance to detail here, further analysis seeming to justify the same result as appears superficially.

fatigue except on such days, and naturally responded to them more when he perceived them than when he did not; while II was considerably subject to them throughout the experiments, especially in the left hand. Then, too, there is no other way of gauging the relative intensity of the fatigue sensations in the two subjects, or the extent of the physiological processes which bring them about. In this connection it must also be noted that I while considerably less subject to fatigue sensations than II is distinctly more subject to actual fatigue losses than II. The figures, as well as those for the 10 subjects, are shown in the following table:

TABLE IX

Relative Fatigability of Right and Left Hands in the Different Subjects

	10 Normal Subjects	Subject I			Subject II		
		Days 1–10	11–20	20–30	1–10	11–20	21–30
Rt. hd.	.912	.890	.882	.870	.915	.912	.922
Lft. hd.	.897	.895	.892	.884	.896	.918	.924

Taking as the unit the average *f* in each experiment, the m. v.'s of the above quoted averages are for the normal subjects .038 and .030 respectively; for the practiced subjects they range between one-fourth and two-thirds as much, being considerably smaller for Subject II.

15. *The correlation of fatigability and initial rate.* A further question presents itself in the relation of fatigability to initial rate. Each individual has a normal initial efficiency and susceptibility to fatigue, and it is the relationships of the variations of the individuals about these norms that we ought to consider. We may first give the relationships as indicated in the records made by the same individual, but on different days. In the two experiments on each of the ten subjects the day of greater initial efficiency corresponded to the day of a greater *f* in but 5% of the cases with the right hand, and in 35% of the cases with the left hand. In the practiced subjects, only the records for the first ten days are considered. In Subject I the correspondence of initial rate and *f* is 0% plus signs for the right hand, and 15% plus signs for the left; in Subject II 20% plus signs for the right hand and 45% plus signs for the left.

In examining now how the relationship subsists between the different series executed by the same subject on the same

day, we find that the negative relationship is here more clear cut than previously. We should indeed expect this, because this is the case in which the data to be compared are obtained under more closely corresponding conditions. We find that the series with the best initial rate of a single record tends also to be the most fatigable series in that record. Perhaps the matter may be more clearly brought out by the following (rather extreme) example, which is from the record of subject E.'s right hand in his second experiment:

TABLE X

No. taps in 1st Interval

	Initial rate.	*f*
1st Series	41	·77
2nd "	36	.87
3rd "	35	·93
4th "	42	·79
5th "	41	.81

It is quite obvious that the 1st, 4th, and 5th series, which have a high initial rate are also in a class by themselves in great susceptibility to fatigue, as opposed to the 2d and 3d series, which have a low initial rate, and show much less fatigue. Calculating the per cent. of like signs relationship for all the records, we obtain an average for the ten subjects of 20% plus signs for the right hand, and 21% for the left; in the practiced subjects, I gives 15% plus signs for the right and 25% for the left, II gives 32% for the right and 20% for the left. This last is the only instance in which the right hand does not give the more pronounced correlation.

We thus observe a general tendency to negative correlation between initial rate and *f*, thus of course a positive one between initial rate and *susceptibility* to fatigue. This is what we should ordinarily expect; "he who runs fast, will not run long."

16. *The effect of practice on fatigability.* The general effect of practice on the *f* have already been observed in Plate IV. In Subject I practice appeared to increase susceptibility; in Subject II there was rather increased immunity. As given in terms of *f*, the results are also seen in the gross by referring

to Table IX. The way in which practice affects fatigue in the different series affords, however, an additional point of some interest. These figures, for the three groups of ten experiments with each hand, are given in the following table:

TABLE XI

Practice and Fatigability

Average *f* for each series in two Groups of ten experiments

Subject I

Rt. Hd.

Days	1–10 Av.	1–10 M.V.	11–20 Av.	11–20 M.V.	21–30 Av.	21–30 M.V.
1st Series	.884	.022	.873	.025	.861	.019
2nd "	.910	.016	.886	.024	.865	.017
3rd "	.884	.035	.887	.025	.866	.016
4th "	.881	.015	.882	.026	.875	.027
5th "	.893	.009	.883	.025	.879	.017
Av.	**.890**		**.882**		**.870**	

Lft. Hd.

Days	1–10 Av.	1–10 M.V.	11–20 Av.	11–20 M.V.	21–30 Av.	21–30 M.V.
1st Series	.893	.019	.868	.018	.865	.019
2nd "	.912	.018	.909	.023	.890	.014
3rd "	.895	.027	.890	.018	.889	.017
4th "	.895	.025	.900	.029	.893	.024
5th "	.880	.032	.889	.018	.885	.017
Av.	**.895**		**.892**		**.884**	

TABLE XI. (Cont.)

Subject II

Rt. Hd.

1st Series	.902	.026	.890	.022	.896	.014
2nd "	.917	.015	.921	.018	.928	.008
3rd "	.915	.021	.920	.010	.924	.018
4th "	.923	.022	.907	.028	.930	.019
5th "	.917	.037	.911	.019	.934	.014
Av.	**.915**		**.912**		**.922**	

Lft. Hd.

1st Series	.892	.026	.904	.012	.910	.012
2nd "	.911	.017	.938	.022	.941	.019
3rd "	.887	.030	.916	.016	.935	.026
4th "	.889	.019	.920	.012	.916	.022
5th "	.901	.017	.909	.021	.914	.015
Av.	**.896**		**.918**		**.924**	

The general tendency of these tables, except always in the left hand of Subject II is that practice establishes throughout an immunity to fatigue that is progressive through the five series. We saw this illustrated in detail in the records of 10 series of one hand above printed in full (Table VII), and we can observe here how this relationship is brought out by practice. The *f*'s uniformly become larger as the later series are reached, and in later practice this tendency is much more marked than at the beginning. It is of course, another phase of the warming up process. Another point is the very low susceptibility to fatigue that is throughout prominent in the second series. By referring to Table V we see that this is also associated in the left hand with a relatively low gross rate,

though not so much as in the right. The reason for this is a rather interesting one. The f, as has been said, is calculated from the initial interval. Now it happens that in the second series, the initial interval is always abnormally slow; always a tap or two slower than the first, and from three to five taps slower than the later series. On the other hand, the warming up process in the later intervals is strikingly in evidence in the second series, and it is this that beside raising the gross rate of the series makes it show immunity to fatigue. This point is also very strikingly shown in Table VII. As may be seen, the uniform tendency in all four records is for the first interval of the second series to be as small or smaller than the first interval of the first series, while the second interval of the second series is as great or greater than the second interval of the first series. The figures are reproduced in Table XII, p. 479.

By comparing the performances during the first two intervals with the total, one can easily see how much more the second series resists fatigue than the first, even when, as in the left hand of Subject I, this resistance is not evident so early as the second interval. Why the first interval alone in the second series should be slower than that in the first series, and the remaining intervals uniformly faster, is difficult to say, but of the general tendency there can be no doubt; it is present throughout the thirty experiments on each practiced subject, as well as quite generally in the ten unpracticed subjects. It would seem as though the warming up affected the later intervals first, and only subsequently extended its effect to the initial one.

On account of the influence of practice in establishing this warming up process, it would seem fairer to judge of the specific effect of practice on fatigability on the basis of the first series only in each record, because these have no warming up series behind them. Referring to Table XI it is evident that the immunity to fatigue as given in f tends with practice to decrease more, or to increase less, in the first series than in any of its followers. Indeed, the 2nd series in the right hand of Subject I, and the 5th series in the left hand of Subject II are the only exceptions to this generalization. It would seem therefore that the specific effect of practice on fatigability is in the nature of an increased susceptibility rather than an increased immunity. The increased immunity shown in the averages of Plate IV and Table IX is largely a product of the increased immunity of the later "warmed up" series. *The true practice gain is one mainly in the initial efficiency of performance, as distinguished from the warming up gain, which shows itself chiefly in continued efficiency of performance.*

It is interesting to compare the effect of practice with that

TABLE XII

	Subj. I, Rt. Hd.			Subj. I, Lft. Hd.			Subj. II, Rt. Hd.			Subj. II, Lft. Hd.		
	0'-5'	5'-10'	Total No. Taps in Series	0'-5'	5'-10'	Total No. Taps in Series	0'-5'	5'-10'	Total No. Taps in Series	0'-5'	5'-10'	Total No. Taps in Series
1st Ser.	40	87	210	89	85	201	41	88	221	86	86	203
2nd Ser.	88	88	210	89	85	204	40	89	228	85	86	204

of age. Both, as may be accepted as certain, tend to increase the rate, and it increases from childhood to adolescence con-

siderably more than it does in the practice of the two adults here tested. In their effects on fatigability, however, the two are somewhat divergent. The effect of increase in age is almost certainly to increase immunity while the above mentioned specific effect of practice, *i. e.*, warming up factors excluded, seems to be to decrease it, or at least to give but very small increase. The modifications in the behavior of the tapping test with increasing age must then in a certain measure be ascribed to different factors than those operative in practice improvement.

17. *Variability.* While no average has been quoted whose m. v. has not been calculated, no great stress has been laid on variability in the preceding paragraphs, and it may therefore have a special mention here. In the present experiments the practically universal effect of fatigue on variability is in the nature of a decrease. This appears between individuals (p. 451) in the records of the practiced individuals from day to day (Table VI, pp. 462-3), and also in the records made by the same individual on the same day. There is so much interaction between warming up and practice that no conclusions regarding a specific effect of these factors on variability can be drawn. There is, however, a general tendency for the m. v.'s of the left hand to be smaller than those of the right.

This last aspect of the results is one that may be compared with certain findings of Bryan; for the most part Bryan's results are not so strictly comparable with the present on account of their averages always dealing with groups, not with individuals. However, there seems also in these findings to be a greater variability in the right hand than in the left. The individual differences in gross rate, which are here expressed in the standard deviation instead of the m. v., show on the whole a greater variability for the right hand; it will be remembered that this was also slightly the case in the present experiments. It also appeared that the year to year fluctuations in rate (among different subjects) were somewhat greater for the right than for the left hand. Although the results quoted can hardly establish the point conclusively, yet it is true that we should probably expect the hand that more nearly reflects the individual's maximum ability to vary more in its ability. In this connection it may be mentioned also that there was no significant difference in the year to year increase in the two hands; this may be compared with the result in the present study that in practice also the right and left hands tended to gain about equally.

18. *The subjective condition as related to gross rate and fatigability.* On each day of the thirty experiments, Subject I graded the general condition introspectively as *A* relatively

good, *B* above medium, *C* below medium, *D* poor. Needless to say, the grade was assigned before the experiment was begun. In all, there were thus assigned 3 *A*'s, 12 *B*'s, 14 *C*'s and 1 *D*. It was desired to see if the grade subjectively assigned tended to correlate in some degree with the gross rate, the fatigability, or other aspect of the test. So far as can be seen, the first relationship is quite chaotic. For example the best performance was made on a *C* day; the performance of the *D* day while not very good in the gross, had the highest initial rate reached yet, and followed three days after an equally poor record made on an *A* day. It would seem therefore that while the daily performances are entirely definite facts and represent such and such a physiological condition with relative precision, this condition is not one that influences the subjective estimate to any appreciable degree. The character of the warming up gain does not seem in any way related to the subjective estimate, nor, it may be mentioned, do the present experiments bear out the correlation suggested in Bolton's figures between general superiority and the presence of interserial warming up. In the index of fatigue, however, a certain relationship is indicated; but, strange to say, it is in the direction of the good days being more fatigable than the poor ones. If we consider only the last twenty days, during which the *f* is no longer subject to practice decrease, as well as the gradings being better distributed, the two remaining *A* days have an average *f* of .86, m. v. 0; the seven *B* days .875, m. v. .010; the ten *C* days .881, m. v. .019. Including the entire thirty days, the *C* average is slightly below the *B*, on account of the low susceptibility of the first few days, uniformly graded *B*; the *A*'s remain at .86. The single *D* day has the lowest *f* recorded, .84; but this is because it came on the 15th day, immediately after a two week's pause, when susceptibility is always greatly increased; it may be mentioned also that this *D* was assigned mainly on the basis of somatic sensations, which were those of marked weariness. If the result is significant, the following are two possible interpretations; the subjective estimate may have been the direct product of physiological conditions which really involved greater susceptibility to fatigue, and, owing to a good subjective condition the subject may have put forth nearer his physiological maximum of effort, and consequently fatigued more. From these considerations it follows as a corollary that the initial rate, or best performance for 5″, corresponds more closely with the subjective estimate than does the gross rate for 30″—on account of the observed relationship of high initial rate, low index of fatigue (*i. e.* high fatigability).

The principal findings of the present study may be thus presented under the various headings as given at the beginning:

1. The extremes of individual variation for 10 unpracticed subjects' rates were in initial rate approximately 2:1, in the gross rates for 30 seconds about 3:2; but it does not appear that this difference is related to general quickness, and beyond efficiency of co-ordination it is not known upon what neural condition it does depend.

2. The index of right-handedness varies between .81 and .94, and though subject to a good deal of variation in the individual, is yet a distinct point of individual difference.

3. With the 2′ 30″ rest-periods used, the right hand of unpracticed subjects tends to increase (warm up) in rate through the five successive series; but this is not so clear in the left hand.

4. After the first few seconds, the rate in the individual series progressively decreases, having, on an average, some six-sevenths as much speed during the last five seconds as in the first five.

5. The fatigue in the tapping test induced by a 30″ performance tends progressively to decrease individual differences in ability for the test.

6. For two normal individuals, the practice curve is everywhere gradual in ascent. The curve fluctuates more from day to day after a week of practice than at the beginning.

7. The left hand does not improve by practice more than the right. Practice tends to increase the m. v. of the five series of each day's performance.

8. Practice brings out the warming up phenomena (*i. e.*, the gain of later series over the earlier) to a marked extent. Next to the gross gain, this is the most important aspect of the practice effect.

9. In Subject I, practice improves the initial interval of the series more than the later; in Subject II all intervals share about equally in the practise gain.

10. Intermissions of two weeks or less have no unfavorable effect on the practice gain, beyond at first increasing fatigue sensations.

11. Warming up shows itself mainly in an increased immunity to fatigue, *i. e.*, the later intervals increase more than the earlier. Its possibilities are by no means exhausted in the five series uniformly taken; it may persist for three or four series further.

12. The ratio of the first to the average of the later intervals furnishes a convenient "index of fatigue" (*f*), through which to observe quantitative relationships in susceptibility and immunity to fatigue.

13. There is only slight tendency to positive correlation in the fatigability of the different hands.

14. The right hand is ordinarily more immune to fatigue than the left; but there are individual exceptions. Practice tends to affect average *f*'s of the left hand more favorably than those of the right. The fatigue sensations are not the product of any factors directly concerned in the speed of repeated movements, and any special correspondences noted between fatigue sensation and actual fatigue loss is the result of reflex inhibition. Of the two subjects, the one more liable to fatigue sensations was less liable to objective fatigue loss. Practice tended to decrease immunity to fatigue in this subject, and to increase it in the other subject.

15. Initial rate and fatigability are negatively correlated, a fast initial rate being usually accompanied by a high fatigue loss.

16. In practiced subjects, the later series of each single record are more immune to fatigue than the earlier, owing to the warming up gain, which affects the later intervals of each series more than the initial. (*Cf.* 11.)

17. Fatigue tends in all respects to decrease variability; no consistent effect on variability due specifically to warming up or practice can be traced. The m. v.'s of the right hand results tend to be larger than those for the left hand.

18. The subjective condition as estimated by an individual practiced in introspection bore no traceable relationship to the gross rate; it seemed, however, that susceptibility to fatigue was greater when the grade assigned was good than when it was poor.

THE STREAMING PHENOMENON

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TABLE OF CONTENTS

Introduction

It is the purpose of this paper to supplement an article published in the *Journal*, in January, 1908,[1] (1) by publication of sketches of the streaming phenomenon, omitted from that article, and (2) by an inquiry whether this phenomenon bears any relation to the various subjective visual phenomena described by earlier investigators.

It will be remembered that the streaming phenomenon was first seen by the writer while observing the fluctuation of the negative after-image. It was confirmed at that time by a number of observers, both trained and untrained, and many have since been added to the list. A brief description is brought forward from the previous article.

"When one sits with lightly closed lids, which must be kept from quivering, before a bright diffuse light such as that of a partly clouded sky, and looks deep into the field of vision thus presented, beyond the background as usually observed, one sees about the point of regard, after the field of vision has steadied, slowly moving swirls. These swirls have the appearance of streams of granules moving in broad curves now this way, now that, seemingly without order, unless a noticeable eye-movement occurs, or is made voluntarily, when the direction of streaming changes to that of the eye-movement. The change of direction is always on a curve, the abruptness of which depends upon the vigor of the movements, much as would happen if motions of different directions and of different magnitude were compounded upon a fluid of considerable inertia. The phenomenon is extremely varied. Sometimes the central portion of the field of vision resembles the surface of a liquid about to boil, channeled this way and that by convection-currents moving at varying rates of speed. Now and again a heavy stream will sweep across this channeled surface from one direction or another, taking up the minor swirls as sharply curving tributaries, and so on, through manifold changes. Various patterns can be picked out, and a particular swirl may be traced in its devia-

[1] C. E. Ferree: *The Intermittence of Minimal Visual Sensations*, XIX, 58 ff.

tions for a time; but, as a whole, the phenomenon cannot be adequately described."

"After practice on the closed lids, the observers became able to trace the streaming on any dull or rough surface with the eyes open." Many were able to see it in this way even before they were able to distinguish it on the field of the closed lids. It "comes out with remarkable clearness with open eyes in the blackness of the dark room. The field of slightly luminous haze that there confronts one . . . . streams and whirls with convincing distinctness."

"It may also be observed under the conditions of observation of the entoptic and circulation phenomena; but just as one must look beyond the false scotomata to see the moving corpuscles and interspaces, so must one look beyond them to see the streaming."

Other conclusions must be briefly called to mind from the previous article. (1) Streaming cannot be an entoptic, circulation, or tear film phenomenon or any of the shadow phenomena, since it is seen in the darkness as well as in the light, and is, besides, radically different in behavior from these phenomena. (2) The direction and vigor of streaming are to a high degree dependent upon the direction and vigor of eye-movement. (3) Streaming has a characteristic effect upon the visual processes. For (*a*) the streams carry with them the visual quality of the background from which they come. (*b*) They tend to obliterate the negative after-image, *i. e.*, gentle streaming dims the after-image and stronger streaming blots it out. Streaming also conditions every color change in the flight of colors. And (*c*), since the negative after-image represents the obverse or recovery phase of the visual processes, streaming brings about the recovery of the adapted retina,—roughly, at least, to the extent to which it increases the fluctuation and decreases the duration of the negative after-image. (4) The streaming material can quite conceivably be identified with the metabolic substance as the vehicle of both the anabolic and catabolic processes. Metabolism requires the diffusion of lymph over the retina. The streaming of this lymph hither and thither brings to the adapted area anabolic material and carries from it catabolic waste. This catabolic waste is probably in part disintegrated visual substance, which retains for a time its power to condition visual sensation (as is shown by the fact that the streams carry with them the visual quality of the region from which they come). Thus, heavy streaming, by weakening the negative process through hastening anabolic change, and by setting up strongly the sensation of the region from which the streams come, may temporarily obscure the after-image. The effect upon the duration of the after-image is probably to be explained wholly in terms of the facilitation of the anabolic process.

Sketches of the Stream Patterns.

The sketches shown in the following cuts were selected at random from a number drawn by the observers. In cuts 1-10 are given illustrations of a few of the more noticeable and easily represented patterns for two observers, Dr. Bair and Miss Alden. In cuts 13-29 are shown the streams that caused the fluctuation in two observations on the connection between streaming and fluctuation. Figures 11-12 represent two stream-types, some form or modification of which was reported by all of the observers.

It will be understood that these sketches do not represent the phenomenon *in toto*, even for these selected cases. They are but a few patterns dissected out, as it were, from the complexity of streaming. It would be impossible to portray even a single cross section of the disturbance, unless the visual field could be fixed for that instant.

Figures 1-6 were drawn by Dr. Bair (*B*). The observation was made near the middle of a clear day in Colorado, October, 1907. The observer sat about three m. from a long window facing the south, and obtained the after-image of a strip of Milton-Bradley yellow paper, 42 by 4 cm., pasted upon a sheet of Milton-Bradley blue, 50 by 60 cm. On the field of the closed lids this stimulus gave a violet strip on a reddish yellow field, a condition especially favorable for the observation of streaming and its effect on the visual processes. This series of sketches is not continuous, representing the fluctuations of one after-image, but is sampled from the observation of several after-images. A break in the outline of the strip in the drawings represents the disappearance of that part of the after-image.

Figure 1 represents a stream which swept to the right and downwards across the after-image, and, turning upon itself sharply in an uncompleted loop, swept back to the left and upwards, blotting out all of the after-image lying between its external borders. In Figure 2 two streams, the one coming from above and the other from below, swept across the after-image from the left in two closely contiguous parabolic curves, blotting out about one-third of the after-image above and below the centre. In all such patterns, when two streams pass near each other, or when a stream turns upon itself, the region between the stream channels is also strongly agitated. This we may call the region of secondary streaming. Figure 3 represents a narrow stream which swept to the right and downwards across the after-image, and, turning, came back upon itself in a broad loop-shaped curve. Two sections of the after-image, each slightly broader than the stream, the one situated above and the other below the centre, were blotted out. In Figure 4 the stream passed across the after-image to the right and, turning broadly upon itself, passed back to the left across the image, and then sharply downwards, losing itself in a broad fan-shaped expansion. Two sections were blotted out; the one short and near the top, the other longer and very irregular in outline,

near the bottom. In figure 5 two streams from the left, the one passing to the right and upward, crossed each other in an X-shaped pattern slightly above the centre of the after-image, blotting out about one-third of its upper half. Figure 6 represents a stream which passed across the image upwards and to the right and, turning upon itself in a vertical oval, passed downward and to the left, along a path nearly parallel to its previous course, cutting out an elongated diamond-shaped section comprising about half the area of the after-image.

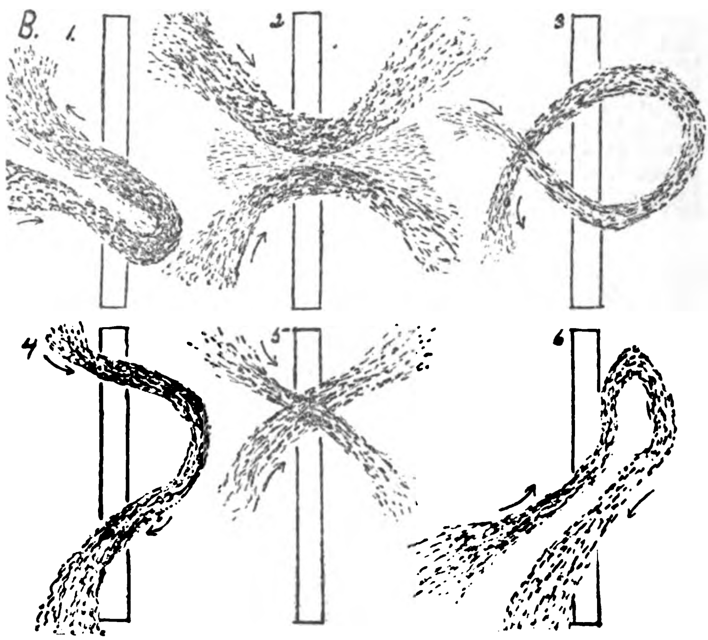

PLATE I

Showing some of the more noticeable and easily represented patterns for Observer *B*. Broken Series.

Figures 7-10 were drawn by Miss Alden (*A*). The conditions for observing 7, 8, and 10 were the same as for the previous observer; but to get the stimulus for Figure 9, the curtain was lowered over the window in front of which the observer sat, and an opening in the shape of a right triangle was cut in it. The sash was lowered and the observer looked through the opening in the curtain at the clear sky. Thus the stimulus was a very bright blue triangle on a dark background.

In Figure 7, first, a stream coming from below and to the left passed across the image, diagonally upwards and to the right. This

was followed immediately by a stream coming from above and to the left which passed diagonally downward and to the right, crossing the path of the first stream in the centre of the field of vision. Before this stream had completed its course, however, it was swept across and taken up by a third stream, moving on a parabolic curve from the right. A large section was blotted out of the central portion of the after-image. In Figure 8, the after-image was swept across, first, by a stream passing downwards and slightly to the right. This was immediately followed by a stream passing upward and slightly to the right. The two streams seemed to form the limbs of a loop, the apex of which was too far advanced into indirect vision to be perceived. Strong secondary streaming was observed to the right of the intersection of the two streams. A large section was blotted out of the centre of the after-image, extending also down the lower left edge. Figure 9 represents a pattern very similar to that of Figure 8, but drafted on broader lines. A wide stream, curving downward and slightly to the right, passed across the triangular after-image, immediately followed by a return stream moving on a similar curve upwards and slightly to the right. This figure also strongly suggested that the two streams were in reality but successive stages of one stream moving in a broad loop-shaped curve. The whole triangle was blotted out,

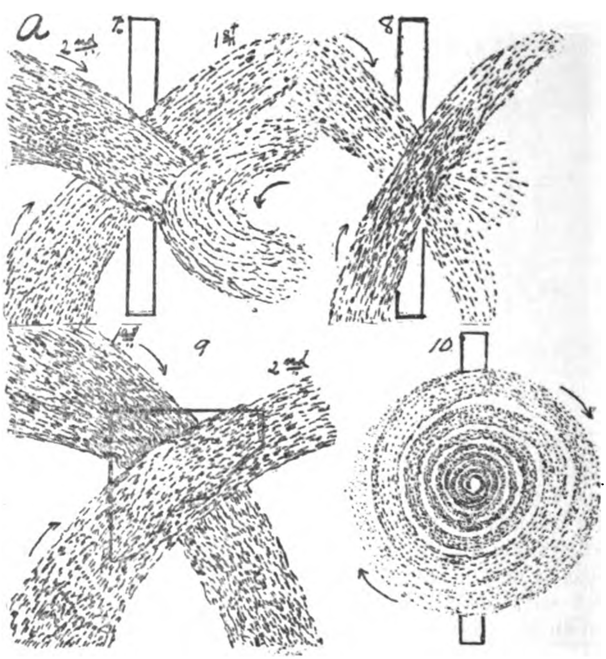

PLATE II

Showing some of the more noticeable and easily represented patterns for Observer *A*. Broken Series.

with the exception of a very small portion of each of the two legs. Figure 10 represents a pattern frequently observed. A swirl small at first begins at the centre of the field of vision and expands centrifugally. It may take in quite a large area of the field of vision before it is swept in this direction or that into a definite stream-form. Expanding as it swirls, and encroaching more and more upon the after-image, this pattern affords a striking and easily observed example of the connection between streaming and the fluctuation of the negative after-image.

Figures 11 and 12 are represented in Plate III.

Figure 12 shows a peculiar pattern, frequently present in some form or other. The stream begins at the periphery in a fan-shaped expansion and moves in a broad curve towards the centre of the field of vision, where it turns sharply upon itself and forms a spiral swirl. At first the movement is slow, but increases rapidly as the stream narrows. Frequently, during the swirling, the movement will reverse and the spiral unwind, sending off a stream towards the periphery which in turn may curve broadly upon itself and return to the centre, thus forming a second swirl similar to the first, and so on through a

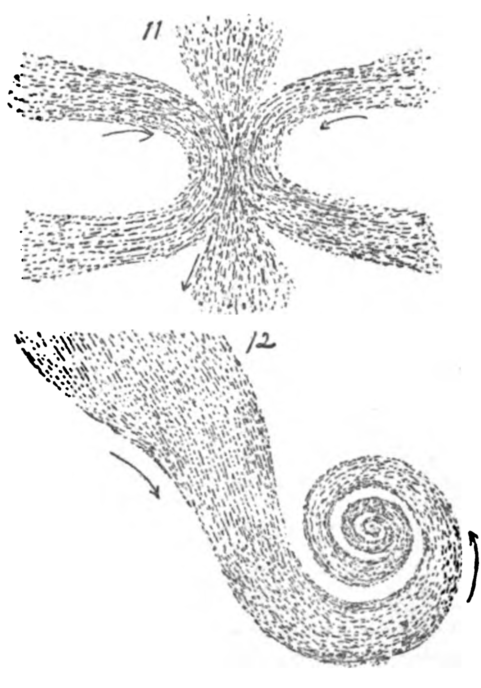

PLATE III

Showing two stream types, some form of which was reported by all of the observers.

number of repetitions. The actual pattern is but inadequately shown in the diagram. Figure 11 shows a very frequent pattern. Two streams moving in opposite directions on a path of slight curvature sweep in towards the centre, and when about to collide turn and complete their paths on parabolic curves. At the apex of each curve the movement is rapid. Strong secondary streaming takes place between the curves, tangent to them at their point of contact, and having the same direction as the primary streaming at this point of contact. In the open this secondary stream is broad and slow, but in the narrow space between the primary streams it is whirled into swift motion. It behaves as if it were drawn in from the surrounding field by the rapid swirl of the primary streams, and were cast out, to lose itself in it again, when the granules of the primary stream enter upon the second part of their course.

Figures 13-21, Plates IV and V, drawn by *A*, represent the streams causing the fluctuation of the after-image of a strip of Milton-Bradley standard yellow paper 42 by 4 cm., from its appearance until its final disappearance. The time of stimulation was 40 seconds, and the distance of the observer from the stimulus was 1 meter. The observation was made in Colorado in October, the observer facing an open window with southern exposure, as was previously described. Only those streams were sketched which caused the after-image to disappear as a whole or in parts.

No streaming was noticeable at first. The after-image was very bright. Gradually a gentle streaming began which dimmed the after-image. Figure 13: a stream moving on a path of slight curvature, downwards and towards the left, swept across the lower part of the after-image. This was immediately followed by a broader stream, of slightly greater curvature, moving downwards and towards the right. The paths of the streams crossed a little to the right of the after-image. The lower third of the image was blotted out. Figure 14: a stream curved downwards and to the left, blotting out about a third of the after-image at the top. Figure 15: a broad, heavy stream, sweeping down from above and to the left, carried the background over the entire image, blotting it out completely. At this point the streaming grew more intensive, the streams became broader and fluctuation became more frequent. Figure 16: a stream sweeping downwards and towards the left blotted out the lower half of the after-image. Figure 17: a broad stream moving almost on the diagonal downwards and towards the left, swept out the whole after-image. Figure 18: a second broad stream, moving downwards and towards the left, but more inclined towards the vertical than the preceding one, passed across the after-image. The streaming was gentle at first, only dimming the after-image, but gaining in intensity soon swept it entirely out. Figure 19: a stream moving towards the right and downwards blotted out the top of the after-image. Before this had passed entirely off the image, a second stream curved downwards and to the left, crossing the first stream to the right of the image, and with it sweeping out the upper two-thirds of the after-image. Figure 20: a broad stream, following the diagonal, swept downwards and to the left across the image, blotting it out completely. Figure 21: a broad stream covering nearly all of the after-image, swept downwards and to the right. Before this had entirely cleared away, a slightly narrower stream came down from

above and the right, crossing the first in an X-shaped pattern. Both streams together blotted out all but the very edge of the bottom and a small portion of the top. From this time on the streaming became still more intensive, and the fluctuation still more frequent. The image would scarcely clear from a previous stream before another swept it out again. The fluctuations were too rapid to fix in mind for sketching. The after-image became dimmer at each reappearance and soon vanished. In all of the above cases the after-image was seen to reappear from behind the stream, as this cleared away, the clearing generally beginning in the direction from which the stream came.

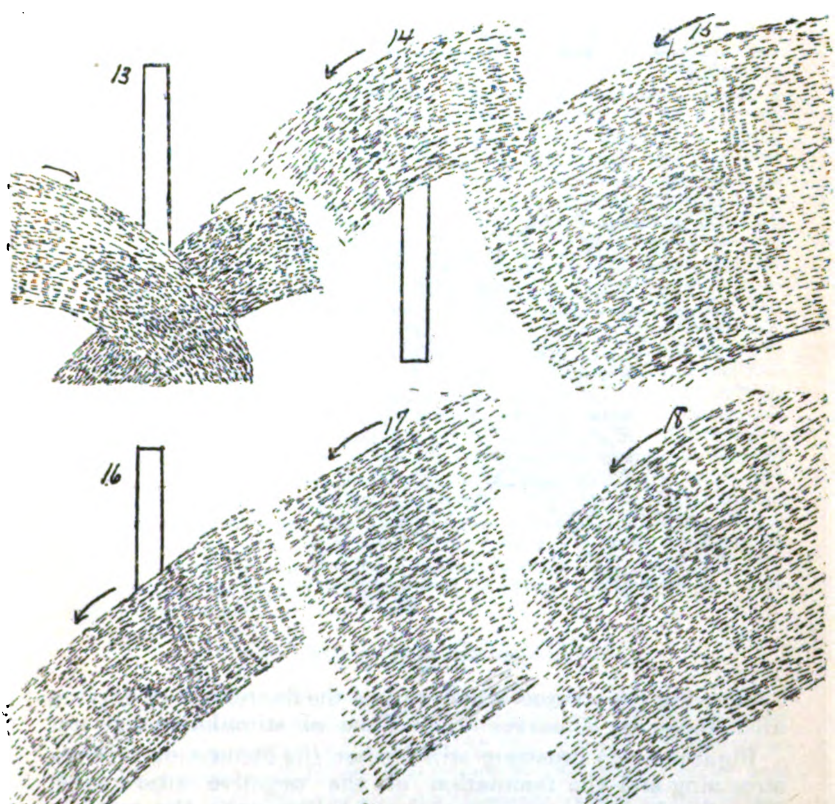

PLATE IV

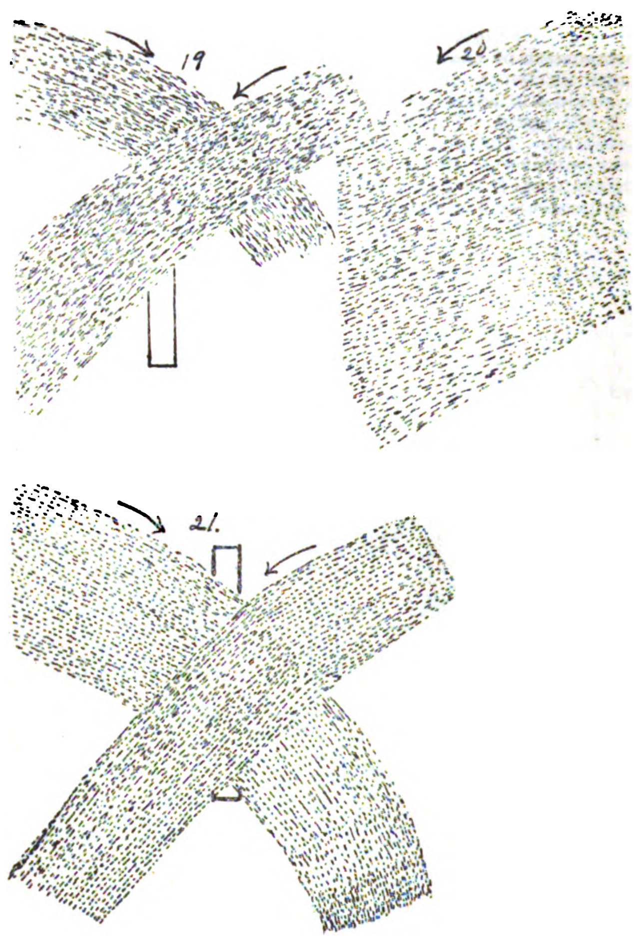

PLATE V

Showing the streams which caused the fluctuation of a given after-image for Observer *A*. Time of stimulation: 40 sec.

Figures 22-29 illustrate still further the connection between streaming and the fluctuation of the negative after-image. The stimulus and experimental conditions were the same in this case as in the former, with the exception that the time of stimulation was only 30 seconds. In a comparison of the two series, a difference will be noticed in the character of the streaming. In the first series the streams were of the nature of

bold, vigorous sweeps of little curvature, such as are caused by eye-movements of considerable range, while in this series they show a more pronounced curvature and are in general more complex in pattern. This difference may be accounted for in part by the difference in the time of stimulation (which permitted the observer a better control of fixation on projection of the after-image), but is probably due largely to the individual difference in steadiness of fixation for the two observers, as shown by their eye-movement records.

There was first a gentle streaming over the whole area of the after-image. A light stream (Figure 22) swept downwards and towards the right, almost, but not entirely, blotting out the after-image. Figure 23: a swirl moving counter-clockwise and coming from above and to the right, was carried across the upper half and centre of the after-image, causing a long disappearance. This is a peculiar and not infrequent form of streaming. Two independent motions are impressed upon the same particles. The granules revolve about a common centre and at the same time are carried forward in a linear path, much as are the atmospheric particles in the vortex of a storm cloud. At times the vortex motion is not so prominent, the phenomenon presenting the appearance of finely powdered snow driven before a brisk wind. Figure 24: two streams, the one moving from the left, the other from the right, joined and passed upwards, blotting out the after-image along its entire length. Figure 25: a stream moving obliquely down and to the left, swept out the lower part of the after-image. Almost immediately another, apparently a continuation of the first, swept up and to the right, carrying out the centre and upper half. Figure 26: a swirl moving counter-clockwise cut off the top of the after-image. Figure 27: a stream moving across the centre from the left, when considerably beyond the after-image, divided and swept back on itself in two broad curves, carrying out the centre, top, and bottom of the image. Figure 28: a stream moving on a very gradual curve to the right, downwards, and to the left, blotted out all but the right edge of the lower half of the after-image. Figure 29: a heavy stream moving across the centre from the right divided and swept back on itself obliquely towards top and bottom, carrying out the whole after-image. From this time on, the visual field was in more or less general commotion. The fluctuation consisted of faint, short appearances, usually of only a part of the after-image at a time, and of long disappearances. The image soon vanished.

The Circulation Phenomenon

In the previous article, the writer showed that the streaming phenomenon cannot be identified with any of the known entoptic or circulatory phenomena of the retina. It devolves upon him here to show that it cannot be identified with any other of the subjective visual phenomena hitherto described, or with any unclassified observations that have been reported in the description of these phenomena or of the circulation phenomenon. To this end it will be necessary to take into account the published accounts of the circulation phenomenon,

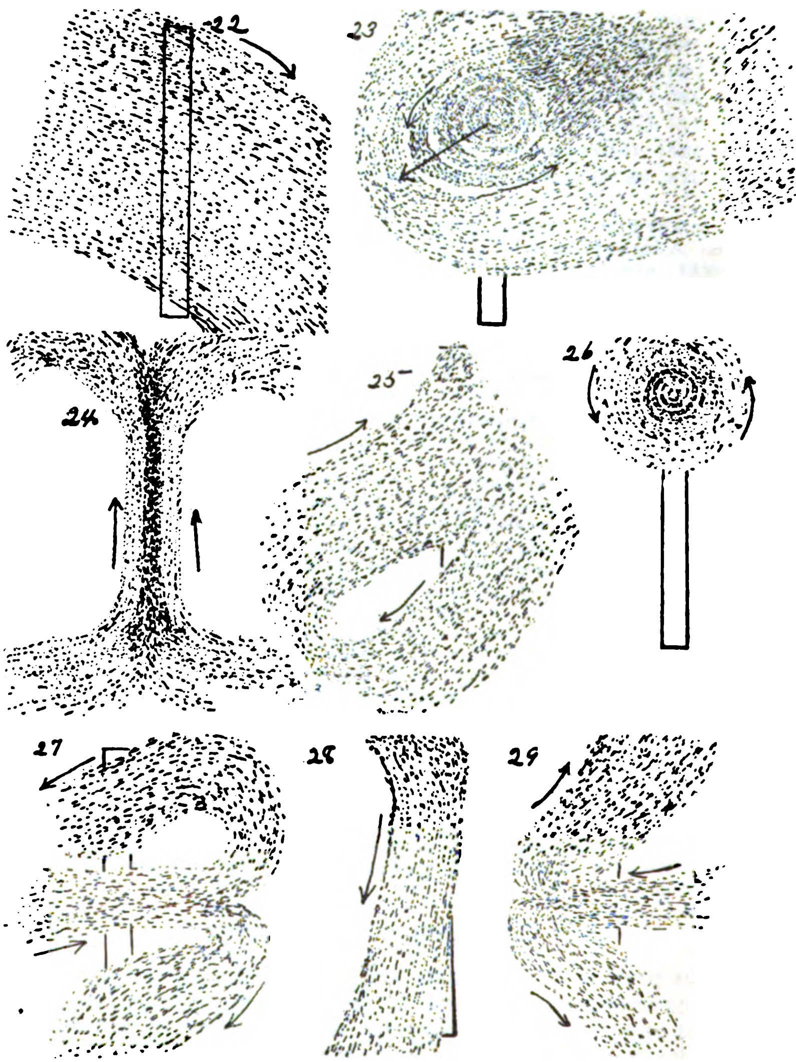

Plate VI

Showing the streams that caused the fluctuation when the after-image was projected with a steadier fixation than obtained for *A* in Plates IV, V.

and of that indefinite group of phenomena called variously *Lichtchaos*, *Lichtstaub*, *wandelnde Nebelstreifen*, etc.

The first reference to the circulation phenomenon appears to have been made by Sauvages.[1] It occurs, however, in his chapter on vertigo, and can scarcely be considered an observation of the normal circulation of the blood. He says: "Si oculis parietem album proximum respicientibus, lucem retro accedentem, solarem non tamen directam sed reflexam recipiat paries, observator videt in muro reticulum obscurum, mox evanescentem."

Darwin, Steinbuch, Purkinje, Ruete, Meissner, Johannes Müller, Vierordt, Rood, Rogers, Aubert, Helmholtz, Odgen and others (see footnote 2, p. 500), have also described the circulation phenomenon; but for our purpose Vierordt's report alone will repay close investigation. The others will be briefly considered merely for the sake of historical continuity.

The first report of the normal flow of the blood through the retinal capillaries was made by Darwin,[2] who states that he could see the moving corpuscles when gazing at the sky or some other bright field, after holding his breath or rubbing his eyes. A few years later Steinbuch,[3] who believed that he was able to distinguish the outline of the vessels as well as the movement of the corpuscles. He found that observation is facilitated by pressure on the eye-ball, when "Reihen von fliessenden Kügelchen" are plainly seen. Purkinje[4] reports that on fixating an extended, well-illuminated surface, bright points are seen in the field of vision, which traverse definite paths at an uniform rate of speed, and appear and disappear at irregular intervals.

Johannes Müller[5] says that a general impression of the movement of the blood may be obtained in many ways; especially if one regards a bright but not shining surface, as the sky, or stares for some time fixedly at the surface of snow or white paper. The phenomenon consists of a confused jumble of intermingling, darting points, or of a vague irregular movement as of mist or smoke, and is so indefinite that not even the direction of movement can be determined.

Ruete,[6] looking at the sky through blue glass, saw many

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[1] Nosol. method., Amsterd., 1763. Tome 3, 242.

[2] R. W. Darwin: New Experiments on the Ocular Spectra of Light and Colors, Philosoph. Transact., LXXVI (2), 1786, 313. See also G. J. Burch, Nature, LIII, 558.

[3] Harless, *Jahrbuch der deutschen Medicin und Chirurgie*, 1813, III., 2, 270.

[4] *Beobachtungen und Versuche zur Physiologie der Sinne I*, 1823, 127.

[5] *Physiologie des Menschen*, 1837, II, 390.

[6] *Bildliche Darstellung der Krankheiten des menschlichen Auges*, 1854, 56.

streams wandering hither and thither, each stream consisting of a single row of alternatingly light and dark points. The points moved in circular paths, convexly curved and frequently intersecting. As pictured by him, the phenomenon presents an appearance very like a hive of bees in the act of swarming. Ruete's description of the circulation phenomenon differs from that given by any other observer;[1] but it nevertheless no more resembles a description of the streaming phenomenon than do the others. The sole point of resemblance to the ordinary accounts of the circulation phenomenon is the mention of "light and dark points" which are seen "in narrow and unchanging paths of motion." This feature alone, however, would be sufficient to differentiate Ruete's observation either from the streaming phenomenon or from the *Lichtchaos*, *Lichtstaub*, etc. Ruete also mentions that the phenomenon is emphasized by the retinal congestion produced as one suddenly rises from a stooping position.

G. Meissner[2] describes two methods by means of which he was able to observe the retinal circulation. When he looked through a small opening at the bright surface presented by the globe of a lamp, with the eye accommodated for a very short distance, the field of vision suddenly darkened and light flecks of irregular form appeared in great numbers. These flecks were very close together, but distinct in outline from the beginning. Motion was gradually set up among them in the most diverse directions, along paths in some places wide, in others narrow, here separating and there reuniting, with the appearance of vessels in which blood corpuscles were rapidly coursing. In the second method, the one eye was closed and the other looked through a lens at the globe of a lamp placed at such a distance as not to give an image, but an intensely bright, uniformly illuminated field. This, when stared at awhile, became dark and slightly tinged with green ("wahrscheinlich in Folge einer Ueberreizung"). In the field of vision thus formed the circulation was plainly visible.

Vierordt[3] distinguishes four stages in the observation of the circulation phenomenon. His method and observation may be described as follows. The left eye was closed and a sheet of milk-glass, uniformly illuminated and placed at a distance of 100-130 mm., was stared at with the right. The fingers, slightly spread apart, were moved rapidly to and fro (120

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[1] Vierordt (*Archiv für physiologische Heilkunde*, 1856, 256), commenting upon this, says: "Ich bin nicht im Stande die Gestalten meiner Blutströmchen in der Ruete'schen Figur auch nur im entferntesten wieder zu erkennen."

[2] *Beiträge zur Physiologie des Sehorganes*, 1854, 84.

[3] *Archiv für physiologische Heilkunde*, 1856, 258.

times per minute or thereabouts) as near as possible to the eyes. After the lapse of a few seconds, never more than a minute, a confused general movement was set up on the white milk-glass surface. This soon resolved itself into numerous light points moving indefinitely hither and thither (first stage), and later into a streaming of the points in definite and fixed directions, but not in channels or paths that were themselves distinguishable (second stage). In a short time, on account of fatigue of the retina, the bright white spaces between the streams became darker. The stream paths themselves, becoming slightly darker, were thus rendered plainly visible. A peculiar state of fluctuation was now induced. Sometimes the visual field would become white again, in which case the stream paths became indistinct (second stage). Then the visual field would again become darker, *i. e.*, become a grayish green, whereby the larger vessels would be made apparent in a brownish color and clearly outlined (third stage). The smaller vessels, however, were still unrecognizable, and the observer received only the general impression of movement, without being able to distinguish the individual corpuscles. Between these two extremes lay an intermitting middle state (fourth stage). The long narrow spaces between the vessels became uniformly light, and frequently also the individual corpuscles could be sharply distinguished as faintly yellowish points. Vierordt says that this fourth stage is that best suited to the observation of the phenomenon, which presents a very surprising and striking appearance, often lasting from 2 to 4 minutes. For observation at this stage the finger movements may be slowed, but cannot be dispensed with entirely.

A difference of opinion seems to exist with regard to Vierordt's observation. Vierordt himself considers it an observation of the circulation phenomenon, and discusses it with that reference throughout. Helmholtz,[1] however, discusses it under the heading *Lichtschattenfigur*, and other systematists, doubtless following his lead, also treat it as a phenomenon of intermittent illumination. Helmholtz, who produces his alternation of white and black sensations by staring at a rotating disk made up of white and black sectors, instead of moving the fingers before the eyes, as Vierordt did, describes the phenomenon as a formless streaming, the direction of which may change as it proceeds, and attributes it to a circulation of lymph rather than to the circulation of the blood. It is chiefly on account of this conflict of opinion, and not by reason of any similarity between this and the streaming phenomenon, that Vierordt's observation and the circulation

[1] Physiol. Optik, 1896, 533.

literature in general have been considered here. Vierordt himself is clear that the streaming is not formless and vague. The third stage might possibly be so construed, out of connection with stages 2 and 4, but in habitually changing to these its character is plainly revealed. Nor do the streams ever shift their paths hither and thither. Whatever Helmholtz may have seen, or however it may bear on this problem, Vierordt's observation cannot easily be misconstrued. It is one of the most definite descriptions of distinct and fixed stream-paths to be found in the literature of the circulation phenomenon. The report throughout conveys the impression of retinal capillaries and interspaces and circulating corpuscles, and Vierordt adopts these terms at the very beginning of his discussion as most fittingly characterizing what he saw.

O. N. Rood recommends the use of cobalt glass for the observation of the circulation phenomenon. Describing it, he says:[1] "The moving bodies at first appeared very closely packed together like fine mosaic work, but as the view grew more distinct their paths could be traced, and the conviction was forced on the mind of the observer, that they were moving at slow uniform rates through narrow channels, the whole reminding one strongly of the circulation seen in the web of a frog's foot through a microscope slightly out of focus." The indistinct vision attendant upon faintness or mere eye-weariness, is, he thinks, in many cases, mediately caused by the appearance of this circulation. For example, he tried the experiment of binding up the right eye and using only the left for two days. After reading with the left eye for half an hour, the page grew indistinct, the letters apparently dissolved, and the dense circulation which he had previously observed was set up, and continued to be visible for some minutes. Under these same circumstances it reappeared several times during that and the following day.

W. B. Rogers,[2] observing in co-operation with Rood, was able to get the phenomenon especially well after long or fatiguing work with the eyes, after active exercise, or indeed, after anything that stimulated the circulation. He describes it as follows: "When I look intently with the naked eye upon a bright surface such as that of a white cloud or a sheet of letter paper in the sunlight, the first appearance presented is that of numerous bright points darting around in various broken curves, coming into view and disappearing fitfully, but in such positions as to indicate the recurrence of the same motions, or the passage of successive particles in certain pre-

[1]Silliman's *Amer. Journal of Science*, November, 1860, 385. See also the September number of the same year.
[2]*Ibid.*, 386.

scribed and permanent channels. As the eyes continue to be fixed on the surface, a shade comes over it, and on the dark ground innumerable streams of particles are seen, moving in infinitely various loops and other curves which, by a little attention, are observed to maintain a constant pattern, strikingly analogous to the capillary circulation as seen under the microscope. These streams of particles are of a tawny yellow as contrasted with the dark brownish surface on which they appear.'' He also gets the phenomenon very well by looking through a black tube at a white surface until the eye has become fatigued, or with the lens of a pocket microscope held at about the focal distance from the eye.

Aubert[1] says that when he presses lightly upon the ball of the eye he can see, with remarkable distinctness, the circulation in the vessels lying close to the yellow spot. The corpuscles move in single file along fixed paths. These paths form very small angles with each other, do not often anastomose, and frequently appear to cross each other without anastomosis. The size of the corpuscles is apparently about fifty times the size of the corpuscles in the capillaries of the frog's foot as seen under the microscope. Pulsations are plainly in evidence but become weaker as the pressure continues.

Describing the circulation as it appears in the visual field of his right eye, Helmholtz[2] reports that it is seen with remarkable clearness in two parallel vessels situated just to the left of the fixation point. The movement is ''scheinbar nach oben gerichtet und das bewegte Gebilde verschwindet, indem es sich mit beträchtlich gesteigerter Geschwindigkeit durch eine S-förmige Krummung hindurchwindet.'' In both of these cases the vessels present a peculiar branched, tree-shaped appearance. The phenomenon often repeats itself, and sometimes in both vessels simultaneously. It occurs also, but less distinctly, at other places in the visual field. Helmholtz explains it as follows. Obstructions are formed here and there by large corpuscles, which dam up the capillaries in their more constricted portions. The corpuscles just in front of these obstructions move on, while those behind crowd against them, thus forming alternating translucent and opaque sections in the capillaries, which are seen as light and dark spaces. These suddenly dart forward or stop, according as the obstruction is overcome or reforms; and so arises the peculiar intermittent, darting movement that characterizes the capillary circulation as seen subjectively, or under the microscope.[3]

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[1] *Physiologie der Netzhaut*, 1865, 342.

[2] *Physiol. Optik*, 198 f.

[3] For other explanation see Rood (*loc. cit.*); Boisser: Landois and Stirling, Manual of Human Physiology, 1895, II, 995; and Ogden's criticism of Helmholtz (*loc. cit.*).

Ogden,[1] working in 1901, in addition to an introspective report, set for his observers the definite task of mapping the course of the blood-vessels. In his report two of these maps are published, showing the paths of both the large and the small streams, also the direction of the movement of the blood streams in them. Describing the phenomenon, his observer *B* distinguishes two classes of movement. In the one, taking place for the most part beyond the macular region, the spots moved rapidly over a considerable distance and scarcely more than indicated their general direction of motion. In the other, near the macula, they moved more slowly over shorter distances and described definite courses. Both corpuscle and path were plainly distinguished. Observations were made also of color and brightness of corpuscle and interspace.[2]

Other Movement Phenomena.

It is, of course, well known that, for the normal eye, the darkened field of vision is not black. It is covered unevenly with shifting, changing clouds of a luminous gray. Purkinje[3] employed the term *Lichtchaos* to cover in a general way this type of phenomenon. A frequent variety, which we shall need to consider here, has been called by Goethe[4] *wandelnde Nebelstreifen*. Purkinje (*loc cit.*) describes it as follows. When one stares into a carefully darkened field of vision, a faint, fantastic image appears and begins to move. At first it is variable and formless, but later it becomes more definite in outline. In general it consists of bands of light and shadow more or less arched, which move

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[1] This *Journal*, XII, 381-391.

[2] Space has not been given to the following reports because they do not add anything of importance to our discussion of those already given.

L. Reuben: On Normal Quasi-vision of theMoving Blood Corpuscles within the Retina of the Human Eye. Sill. Journ. (2), XXXI, 1861, 325-338. *Ibid.*, 417.

B. A. Pope: Entoptische Erscheinungen im Zusammenhang mit dem Blutkreislauf, Arch. f. Augen- u. Ohrenheilkde, I, 1869, 72-78, *Ibid.*, 459.

B. Becker: Ueber die sichtbaren Erscheinungen der Blutbeweg ungen in der menschlischen Netzhaut. Berlin, 1872.

S. E. Ayers: Der Blutlauf in der Gegend des gelben Flecks. Arch. of Opthalm. XI, 1882, 476. Arch. f. Augenheilkde. XIII, 1883, 29.

J. W. Barrett: Nature, LIII, 510.

G. J. Burch: *Ibid.*, 558.

For methods of mapping the retinal circulation see Ayres (*loc. cit.*); Ogden (*loc. cit.*); Randall, in deSchweinitz, Amer. Text-Book of Diseases of the Eye, Ear, Nose and Throat, 1889, 140; and O. W. Maher, Austral. Med. Gaz., IV, 38.

[3] *Beobachtungen und Versuche zur Physiologie der Sinne*, 1823, I, 58.

[4] *Farbenlehre*, Abth. 1, § 96.

"als concentrische Kreise gegen den Mittelpunkt des Sehfelds, und dort sich verlieren, oder als wandelnde Bogen an ihm sich brechen und in sich selbst zusammenkrümmen, oder als krumme Radien um ihn im Kreise sich bewegen." Its movement is slow. Eight seconds are usually required for one of the bands to run its course and disappear. From the beginning of the observation the darkness is never absolute. The field of vision is filled with a boundless chaos of weak light.

Observing more in detail, Purkinje distinguishes four cases of the phenomenon, which are described as follows. (1) A weak light in centripetal motion is seen in the centre of the field of vision. Surrounding this is a black band, which in turn is bounded peripherally by a belt of dull light. These move inward, and seem to displace the central luminous spot as a round dark area. Next the whole pattern (luminous ring, dark band, and faintly luminous, shimmering fringe) re-forms about this dark area, and, moving inward as before, takes its place. See Plate VII, figure 17.

(2) Broad luminous and broad black bands form alternately above the centre and move towards it. As they proceed they bend to form arcs which grow smaller and smaller, uniting into a single light-mass, and finally disappearing at the centre. See figure 18.

(3) Analogous to these are other patterns in which the light and dark bands extend either vertically or obliquely, and move towards each other. See figure 19.

(4) A fourth form consists of two-curved bands which extend from the middle point of the visual field in opposite directions, and turn about it. Later, after the attention has become fatigued, these patterns give place to irregular waves of light and shadow. Finally, these subside and there appears to be drawn over the visual field a scarcely perceptible veil of dull light.

18 19

17

PLATE VII

Showing Purkinje's drawings of the *wandelnde Nebelstreifen:* drawings 17, 18, 19.

Johannes Müller[1] describes the subjective lighting of the visual field somewhat differently. "At times the phenomenon appears as a general illumination of the field of vision. Again it takes the form of a bright shimmer, which spreads out in circular waves towards the periphery and vanishes. Frequently the shimmer is flecked, cloud-like and nebulous. And very rarely the phenomenon repeats itself with a sort of rhythm."

Volkmann[2] says that, when the eyes are closed and covered, the field of vision is seen to be filled irregularly with a sort of *Lichtstaub* which is present in different amounts under different circumstances. (Fechner, for example, after his prolonged investigation of the physiological colors got it in such excessive amounts for a year that he called it "*ein Lichtmeer*".) Color phenomena are also perceived, the colors, varying at different times. An observation made by Volkmann soon after awaking in the morning is reported as follows: "There was at first a shadow-field containing a small amount of light-dust which streamed from left to right. This light-dust increased, and a reddish centre formed in the shadow-field. The reddish centre became quadrangular in shape and spread out gradually over the whole visual field. In the centre of the red field thus formed, a green spot appeared, which likewise expanded, until it covered the visual field. Again a red spot developed at its centre, and spread as before. The red field of vision was thickly dotted or pointed, the points streaming from left to right." Sometimes violet and green were seen instead of red and green, and their order of succession frequently varied.

Aubert[3] describes the subjective light phenomena of the darkened field of vision as follows: (1) The field is never free from points and lines of light which move in a slow peculiar fashion. In appearance the patterns formed can be compared to floating strings of tow. The light mass is yellow in color, is not especially bright, and is variable in amount. (2) Goethe's *wandelnde Nebelstreifen* are often seen floating over the field in various directions. They are indefinite in form, and colorless. Eye-movement or blinking either hastens their movement or causes them to vanish. (3) At times, when the eyes are held especially steady, nebulous balls appear in the middle of the visual field. They alternately expand and contract, but do not move from place to place. They are brightest at the centre and shade off formlessly towards the periphery into the dark background. (4) Very bright points of light appear suddenly and vanish quickly. These points

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[1] *Spontane Lichterscheinung im dunkeln Sehfelde:* Physiologie des Menschen, 1837, II, 391.
[2] *Wagner's Handwörterbuch der Physiolgie*, III, 1846, 311.
[3] *Physiologie der Netzhaut*, 1865, 333 f.

are so bright as to be mistaken for objective light. Under the most favorable conditions for observation, they cannot be seen more than a few seconds. (5) And fifthly, zigzag lines are sometimes observed, appearing either as bright, bluish, or violet flashes.[1] They move slowly and vanish after a few seconds. Aubert mentions also rotating shapes resembling comets, colored clouds, and "das eigenthümliche Strahlenschiessen von der Peripherie nach dem Centrum" which Ruete[2] has described. He says that long exposure to dark, so far from interfering with these phenomena, rather serves to intensify them.

Again, Helmholtz[3] reports the *wandelnde Nebelstreifen* as two systems of circular waves which slowly converge from both sides of the visual field towards its middle point. The locus of this middle point, he says, seems to be the entrance of the optic nerve. A certain periodicity obtains which, as it appears to him, roughly coincides with the respiratory rhythm. Lastly, Pierce[4] describes what he calls the "illusory dust drift." When one fixates a set of parallel white and black lines in a strong light for 20 sec. or more, and then transfers the gaze quickly to a square of dull black cardboard, the illusory dust drift may be seen. The appearance is that of a cloud of fine white dust moving across the field of vision in a direction in general perpendicular to the direction of the lines in the stimulus. When the stimulus is an uniform field, instead of lined, the drift does not occur. Pierce explains the phenomenon as an after-image of motion. The parallel lines solicit the eyes to move in a direction perpendicular to them. The numerous involuntary eye-movements thus set up give rise to after-images of motion when the eyes come to comparative rest on the uniform field of the black cardboard.—

It seems clear from this review that the writer was justified in regarding as new his observation of the streaming phenomenon. None of the movement phenomena mentioned agree in any essential particular with the streaming phenomenon. There are disturbances in the field of vision, secondary to streaming and out of the paths of the streams, which may possibly bear some relation to the shifting clouds, etc., of the retinal light; but there is no evidence that the streams themselves, with their definite stream forms, their shifting paths, and, still more distinctive, their characteristic effect on the visual processes, have hitherto been observed.

[1] For a description of the *Zickzacklinien* see also Purkinje, *Beobachtungen*, II, 84.

[2] *Lehrbuch der Ophthalmologie*, 1845, 72, figure 39.

[3] *Physiol. Optik*, 242.

[4] Studies in Space Perception, 331-38.

THE EFFECT OF SUGGESTION UPON THE REPRODUCTION OF TRIANGLES AND OF POINT DISTANCES

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Historical

It is generally recognized that every one is more or less influenced by suggestion. Experimental studies to determine the exact nature and amount of this influence, however, meet with considerable difficulty, owing to the artificial conditions which necessarily prevail in the laboratory. Especially is it difficult to devise a method of procedure which will afford an accurate measure of the effect of suggestion upon the reaction to visual stimuli.

Small,[1] in his study of the suggestibility of children, had a toy camel arranged with a rope around its neck in such a way that when he turned a windlass the suggestion was given that he was pulling the camel, though the latter did not move. Influenced by the suggestion 291 out of 381 pupils asserted that they saw the camel move. The percentage of effectiveness of the suggestion varied in the different grades tested as follows:

Grades	I	II	III	IV	VI	VII	Total
Percentage of illusion	96	81	80	70	34	71	76

The low percentage for VI is accounted for by the fact that the conditions were unfavorable for the illusion. In this experiment the mechanism of the windlass and the force of its pull were carefully explained beforehand and a high degree of expectation aroused, so that the asserted perception of movement was the result of the vivid idea.

Binet[2] reports an experiment, also on children, which is more closely allied to the present investigation. A line 50 mm. in length was shown by the director of a school to each pupil individually and the pupil was required to draw a line of equal length. In these reproductions there was a marked tendency to draw the line too short, seventy-eight out of eighty-six making it less than 50 mm. The reproductions varied in length from 28 mm. to 60 mm. The director then stated that

[1] Small, M. H.: The Suggestibility of Children, *Ped. Sem.*, Vol. 4, 1896, p. 182.
[2] Binet, A.: La Suggestibilitè. *L'Annèe Psych.*, Vol. 5, 1899, p. 99.

they would be shown a *longer* line, which they were to reproduce in like manner. The line which they were shown, however, was actually shorter, measuring only 40 mm. Only nine of the eighty-six drew the second line shorter than the first, and of these only one drew it 10 mm. shorter. In the case of the seventy-seven who followed the suggestion, the increase in the length of the second line over the first varied from 0 to 20 mm., with an average of 6 mm. The younger pupils showed a greater percentage of suggestibility than the older. The director then told them that they would be shown a third line a little *shorter* than the second, whereupon the original line of 50 mm. was presented again. This suggestion was less efficacious than the first, only seventy out of the eighty-six yielding to it. The amount of error was likewise reduced. Whereas in the first suggestion thirty-three showed a positive error of 5 mm. or more over the length of the original 50 mm. line, in the second only twenty-three showed as great a negative error below the 40 mm. line.

Pearce[1] tested the influence of a suggestive stimulus upon the extent of eye movements as indicated by visual localization. The subject, seated at the centre of a circle whose radius was 3½ feet, fixated a small square of white paper on the periphery directly in front of him. After 2 seconds another square was shown for 2 seconds at, *e. g.*, 30° to the right of the fixation point, and on its disappearance the subject turned his eyes to the point and indicated its location. As a suggestive stimulus a third square was shown 15° to the right or left of the second, the subject still keeping his gaze directed to the fixation point, and after the suggestive stimulus had disappeared the subject indicated the location of the second square. Five points were chosen from 30° to 69° to the right.

Without a suggestive stimulus there was an error of —0.4° to —6° (mean —3.2°), that is, the point localized was always to the left of the stimulus, the line of regard did not move far enough. When the suggestive stimulus was shown 15° to the *left* of the stimulus to be localized, the average error was —0.2° to —7.4° (mean —4.4°), showing a decided positive influence of the suggestion. When the suggestive stimulus was 15° to the *right* of the peripheral stimulus the average error was +1.2° to —6.5° (mean —2°), again showing a positive influence of the suggestion. Summing up his results Pearce notes that with a single peripheral stimulus the error toward the fixation

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[1] Pearce, H. J.: Normal Motor Suggestibility. *Psych. Rev.*, Vol. 9, 1902, pp. 348-355.

point increases with the distance of the peripheral stimulus from that point. With the suggestive stimulus there is at first a tendency to resist the suggestion, but this diminishes as the suggestion is repeated. The resistance is most vigorous when the suggestion is contrary to the normal error, but ultimately the suggestion opposed to the normal tendency is most effective. The same person showed the highest degree of suggestibility in all of Pearce's tests, whether with visual, auditory or tactual stimuli.

The work which suggested the present investigation was that of Brand,[1] who tested the suggestive effect of printed mottoes on the reproduction of horizontal lines. These lines varied from 12 to 34 cm. in length and were represented by the interval between two pegs situated at a distance of 120 cm. from the eyes of the observer. The observer reproduced this interval by spacing two similar pegs on a ledge 40 cm. from his eyes. Of the four observers whose results are presented the first overestimated the distance when nonsense mottoes were shown, the second and third underestimated it, while the fourth reproduced it almost exactly. The effect of the "short" suggestion (mottoes containing the word "short," or "make short," or "make short enough") upon the first subject was to produce a slight negative error in the aggregate, while the "long" suggestion considerably increased the positive error. In the case of the second subject "short" suggestion caused a slight increase in the underestimation, while "long" decreased it. With both subjects the "long" suggestion was more effective than the "short." The fourth subject showed a positive error for "long" and an equal negative one for "short" suggestion. With all three of these subjects, therefore, the suggestion was positively effective. In the case of the third subject, however, "long" suggestion caused an increase in the underestimation, while "short" caused a decrease, *i. e.*, the effect of the suggestion was negative, the results being opposite to the nature of the suggestion. Here again the "long" suggestion was more effective than the "short."

More recently Smith and Sowton[2] have investigated the effect of what they called successive contrast. A modifying line varying from 2 to 20 cm. was shown and immediately afterward a standard line of 10 cm. was exposed. The length of the standard was marked off on a line already drawn on a sheet of paper. In all cases, both with and without the modi-

[1] Brand, J. E.: The Effect of Verbal Suggestion upon the Estimation of Linear Magnitudes. *Psych. Rev.*, Vol. 12, 1905, pp. 41-49.

[2] Smith, W. G. and Sowton, S. C. M.: Observations on Spatial Contrast and Confluence in Visual Perception. *Brit. Journ. Psych.*, Vol. 2, 1907, pp. 196-219.

fiers, the standard was underestimated, but for one of the two subjects modifiers of 2–10 cm. produced an average increase of 1.2 mm. in the estimation, while modifiers of 10-20 cm. caused an average decrease of 0.9 mm. That is, the shorter lines acted as a positive suggestion, the longer ones as negative.

Experimental

In the experiments now to be recounted the following apparatus was used. Upon a long table rested a black-lined box in the form of a truncated pyramid 8 × 12 cm. at the small end, 65 cm. square at the large end, and 185 cm. long. The large end was covered with black cardboard in the centre of which was an opening 25 cm. square, closed by a screen which could be raised and lowered at the pleasure of the operator. 15 cm. behind this screen was placed a frame to hold the cards upon which were drawn the forms to be reproduced. These cards were illuminated from the side by a four candle-power incandescent light, to secure constancy of illumination. The observer, seated at the end of the table, rested the forehead against a head-rest fastened to the small end of the box, and looked through the box at the cards. The latter were thus two meters from the eyes of the observer. The time of the exposure was two seconds.

Two kinds of suggestion were used, auditory and visual. The auditory suggestion consisted of a command, such as "make high," "make low," "make high enough," etc., or simply "high," "low," uttered in a firm tone of voice at the moment of exposure. The observers were requested to oppose no active resistance to the command, to allow their attitude to be as passive as possible so that the suggestion might have a chance to manifest its effect, yet they were asked to reproduce the figures as accurately as they could. For the visual suggestion a diamond-shaped figure 20 cm. long and 4 cm. wide (Plate I, Fig. XI) was shown on the frame, and immediately thereafter the form to be reproduced was exposed. As this diamond when shown in a vertical position was considerably higher than any of the forms, and as the shape of the figure was such as would probably cause an up and down movement of the line of regard, it was thought that it might serve as a "high" suggestion. On the other hand, when shown in a horizontal position the diamond-shaped figure gave a flattened out effect, so that it was called a "low" suggestion. To ensure this figure remaining in consciousness during the reproduction the observer was required to draw it with the greatest possible degree of accuracy as soon as the reproduction was made.

The forms decided on for reproduction were *A*. Triangles of different shapes and heights. *B*. Vertical point distances,

(1) Dots at given distances above the centre of a base line,
(2) Dots at like distances above another dot.

A. Triangles

Ten triangles of different shapes and heights were used, all having the same base of 10 cm. (See Plate I.) They varied from 49 to 100 mm. in altitude. Four were isosceles or nearly so, four were right or slightly acute, and two were obtuse, their apices projecting to the right of the base. The observer was given a sheet of paper 16×22 cm. with a base line of 10 cm. already drawn upon it, and was required to indicate with a pencil dot where the apex of the triangle would fall. Nine reproductions of each triangle were made with each kind of

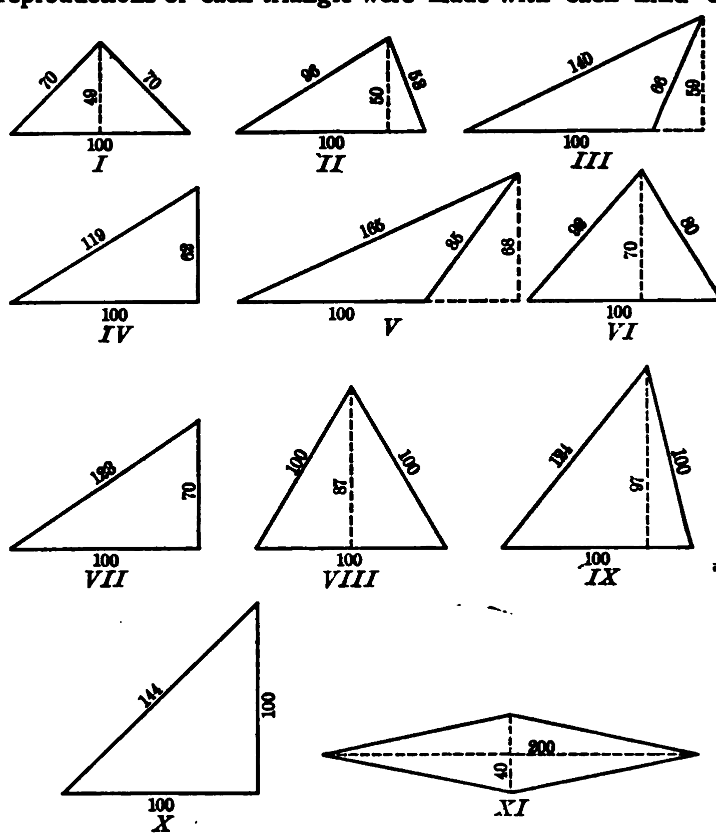

Plate I

The Arabic figures give the dimensions of the standard Triangles in millimeters.

suggestion, auditory and visual. The ten triangles were presented to the observer one after another in irregular order, and this constituted a series. Thus nine series composed a set. In order to obtain standards of reproduction for each observer without suggestion the first, fourth and seventh series of each set were given without suggestion, the triangles being presented in chance order. By taking the averages of these series as standards the practice element was practically eliminated. In the remaining series high or low suggestion was given in such a way that, while the order of the individual suggestions was quite irregular, when the set was finished each triangle had been shown three times with high suggestion and three times with low. Each leg of the reproduced triangle was measured and the altitude computed. This altitude was then compared with the altitude of the original triangle and the excess or deficiency expressed in millimeters as a positive or negative error. Six persons participated in this part of the experiment as observers, and are designated by the letters A, B, C, D, E and F.

Table I represents the average error in altitude of three reproductions of each triangle under the conditions of standard, high suggestion and low suggestion, for the two types of suggestion, auditory and visual, which were employed. The Roman numeral indicates the number of the triangle, and is followed by the actual altitude of each triangle expressed in millimeters. At the bottom of each column is indicated the algebraic sum of the average errors for all the triangles under the conditions indicated. This will be referred to as the aggregate error for each observer under the condition specified. To the right are columns showing the algebraic sum of the average errors of all observers for each triangle. This represents the aggregate error for each triangle under the conditions indicated. In the last column to the right is given the average error per observer for each triangle in the standard experiments, taken without regard to the direction of the error. This will be referred to as "average error of standard."

In inspecting this table it must be borne in mind that the observations of the auditory set were completed for all observers before the visual set was taken up, so that the latter tests were made several weeks later than the former. This fact, as well as the different attendant conditions (the one was taken in the midst of auditory, the other of visual suggestions) might be mentioned to account for the difference in standards which is observed in the two sets. If we try to compare these results of different observers for individual triangles we find great variation in the way the observers react to the same situ-

TABLE I. TRIANGLES

Auditory

No.	Alt. mm.	A. St.	A. Sugg. H	A. Sugg. L	B. St.	B. Sugg. H	B. Sugg. L	C. St.	C. Sugg. H	C. Sugg. L	D. St.	D. Sugg. H	D. Sugg. L	E. St.	E. Sugg. H	E. Sugg. L	F. St.	F. Sugg. H	F. Sugg. L	Aggregate St.	Aggregate Sugg. H	Aggregate Sugg. L	Ave. Error of St.
I	49	+9	+8	+1	-3	+13	-8	0	-2	-2	+10	+5	-3	+14	+9	+8	+2	-1	+1	+32	+32	-3	6.3
II	50	+2	+7	+5	-1	+6	-8	-3	-6	-7	+1	-5	-5	+1	+4	+2	-1	+2	-2	-1	+8	-15	1.5
III	59	+17	+18	+3	+2	+10	-2	-1	+6	-1	+7	-1	-6	+8	+15	+21	0	-1	-2	+33	+47	+13	6.
IV	62	+10	+9	+3	-2	0	-12	-1	-1	+6	+2	+2	-5	+7	+9	+3	-5	+4	-6	+11	+23	-10	4.3
V	68	+22	+18	+15	+3	+11	-9	+22	+1	+4	+1	+18	-7	+9	+18	+11	-2	+7	-3	+53	+73	+11	10.
VI	70	+11	+13	+10	+10	+8	-8	+4	-4	-4	+1	-1	-5	+10	+9	+4	+5	+7	+4	+41	+32	+1	7.
VII	70	+12	+6	+2	-10	+6	-12	-1	+1	-5	+8	+3	-6	+3	+6	-1	-1	-1	0	+11	+21	-22	5.8
VIII	87	+26	+8	+19	-4	+13	-9	+11	+1	+5	+7	+3	-8	+7	-1	+3	+2	+4	+4	+49	+28	+14	9.5
IX	97	+7	+9	+2	-9	+8	-20	-13	-8	-11	-10	-14	-10	0	+4	-3	-7	-11	-8	-32	-12	-50	7.7
X	100	-1	0	+5	-16	0	-28	-21	+4	-16	-8	-6	-15	+2	+3	+9	-12	-14	-7	-56	-13	-52	10.
Total		+115	+96	+65	-30	+75	-116	-3	-8	-31	+17	+4	-70	+61	+76	+57	-19	-4	-19	+141	+239	-113	

Visual

No.	Alt. mm.	A. St.	A. Sugg. H	A. Sugg. L	B. St.	B. Sugg. H	B. Sugg. L	C. St.	C. Sugg. H	C. Sugg. L	D. St.	D. Sugg. H	D. Sugg. L	E. St.	E. Sugg. H	E. Sugg. L	F. St.	F. Sugg. H	F. Sugg. L	Aggregate St.	Aggregate Sugg. H	Aggregate Sugg. L	Ave. Error of St.
I	49	+5	+11	+6	+3	+15	-3	+3	+10	+9	+7	+5	+10	+2	+6	-11	+4	+4	+7	+24	+51	+18	4.
II	50	+4	+3	-1	+2	+20	+1	+3	+2	-2	+2	0	+3	+3	+2	+2	+1	+1	0	+15	+28	+3	2.5
III	59	+12	+3	+18	+5	+25	+10	+18	+14	+10	+7	-2	-5	+4	+4	-5	+2	+7	0	+48	+51	+28	8.
IV	62	+6	+8	+5	0	+16	+8	+7	+6	+12	+6	+3	-1	-2	-1	+2	-1	-1	+2	+16	+31	+28	3.7
V	68	+11	+31	+16	-3	+11	-7	+6	+15	+14	+7	+18	+23	+16	-21	+2	+4	+9	+6	+41	+63	+54	8.
VI	70	+11	+6	+11	-6	+14	0	+7	+8	+3	+2	-1	+4	+1	+2	+4	-2	0	+4	+13	+29	+26	5.
VII	70	+3	+5	+3	-2	+9	-8	0	+11	+13	+1	-4	+2	+3	-1	0	-5	-1	0	0	+19	+10	2.3
VIII	87	+13	+12	+3	0	+7	-4	+14	-1	+27	+13	+4	-1	0	-14	-6	+1	+3	+5	+41	+11	+24	7.
IX	97	-4	+5	-6	-11	+3	-23	+13	+4	-11	-8	-16	-15	-8	-9	-12	-5	-15	-7	-23	-28	-74	8.2
X	100	-3	+2	-9	-16	+2	-25	+19	-7	-2	-9	0	-7	0	-15	-7	-12	-12	+7	-21	-30	-43	10.
Total		+58	+86	+46	-28	+122	-51	+90	+62	+73	+28	+7	+13	+19	-47	-31	-13	-5	+24	+154	+225	+74	

ation. If we consider the aggregate error of all the observers for each triangle, however, some interesting things appear.

1. Triangles II, IV, and VII show the lowest aggregate as well as the lowest average error in the determination of the standards in both auditory and visual sets. These triangles were 50 mm., 62 mm. and 70 mm. in altitude, and their right base angles were 70°, 80° and 90° respectively. This agrees with the assertion frequently made by the observers that the right triangles seemed easier to reproduce and they felt that the results were more accurate than with the other triangles. In X, however, another right triangle with an altitude of 100 mm., the widest variation is shown, and the aggregate error is strongly negative. The next widest variation is found in V, an obtuse triangle with an altitude of 68 mm., where the aggregate error is as strongly positive. In general the obtuse triangles III and V were said by the observers to be the most difficult to reproduce and the results were felt to be the least accurate, owing, perhaps, to the lack of base line beneath the apical point. In triangles I, VI, VIII, and IX, which vary but little from isosceles, there is a considerable error both aggregate and average. In all cases, except the last, the aggregate error is positive, in IX, with an altitude of 97 mm., the error is as strongly negative.

2. If we consider the result of high and low auditory suggestion upon the aggregate error of each triangle, we observe that in all but three triangles (I, VI and VIII) the high suggestion produced an increase in the aggregate error over that of the standard, *i. e.*, was positively effective, and that in one of these three the amount remains the same. For the low suggestion the results are even more uniform, all triangles but one (X) showing a decrease below the standard. The total aggregate error for all the triangles shows an increase of 98 mm. above the standard for high suggestion and a decrease of 254 mm. below the standard for low suggestion.

3. The results for visual suggestion are not so uniform. Three triangles (VIII, IX and X) fail to show any increase for high suggestion, while four (IV, V, VI, and VII) fail to show a decrease for low suggestion. The totals for all triangles, however, show an increase of 71 mm. for high and a decrease of 80 mm. for low suggestion as compared with the standard.

In so far, then, we may conclude that in general the suggestions do affect the reproductions of the triangles, that the auditory suggestion is more effective than the visual, and that in the auditory set the low suggestion is more effective than the high.

But the chief interest of this investigation lies not in the

way in which this group of observers reacts as a whole to individual triangles, but rather in the effect of high and low suggestion upon the reactions of each observer. Here again we shall consider the nature and amount of the aggregate error of all the triangles for each observer in each situation.

1. Auditory set. Referring to Table I we note that the total aggregate errors for each observer are as follows:

TABLE II. AGGREGATE ERRORS. AUDITORY

	Standard	Suggestion High	Suggestion Low
A	+115	+96	+ 65
B	— 30	+75	—116
C	— 3	— 8	— 31
D	+ 17	+ 4	— 70
E	+ 61	+76	+ 57
F	— 19	— 4	— 19

An inspection of the standard shows that observers A, D and E exhibit a positive type of error, *i. e.*, tend to place the point higher than it should be. C in this particular set shows a slightly negative standard (—3), but as in all the other work C's standards are decidedly positive it is better to consider this as due to the abnormally large negative error which C shows in reacting to triangles IX and X, and to call C positive also. B and F are negative, *i. e.*, place the point too low.

Turning to the effect of the suggestion, we find that in every case the aggregate error for low suggestion lies below that for high, and in every case but one it lies below the standard. In the one exception (F) it just equals the standard. The amount of decrease below the standard varies from 0 to 87 (A 50, B 86, C 28, D 87, E 4, F 0). The low auditory suggestion, therefore, seems to have been effective with all observers. The high suggestion does not seem to have been so generally effective. In three cases (A, C, and D) the aggregate error fell below that for the standards, while only in the case of B (—30+75=105) did it rise far above the standard.

Visual set. The same types of reaction to the standards prevail here as in the above set. The low suggestion is effective in five cases in reducing the error below that for the standards. In the sixth case (F) there is a rise of 37. For the high suggestion the error fell below that of standards in three cases again (C, D, and E), and in all these cases it was even lower than the results for low suggestion. B again shows a strong rise (+150) above the standard.

In summing up the work on the triangles we may say that observer B alone showed striking susceptibility to the suggestion in all cases; that the susceptibility to low suggestion was

more general and more uniform than that to high; that A showed a considerable positive error throughout, and but for a few very low reproductions in the high suggestion would have shown a uniform susceptibility; that with three observers in each series any kind of suggestion, whether high or low, tended to reduce the estimate below that of the standards; and that in the visual series the reduction for high suggestion went even farther than that for low. Upon C, D, and E, therefore, it would seem that the vertical diamond-shaped figure did not act as a suggestion at all, but that its exposure before the triangle to be reproduced gave rise to successive contrast, *i. e.*, caused the following triangle to seem lower than it was. Yet the figure, when placed horizontally, was in as sharp contrast to the height of the triangles as when vertical, and in this situation its effect was in the nature of a low suggestion.

The observers were inclined to doubt whether either the auditory or the visual suggestion was having any effect upon their reproductions, but thought that if either did so it would be the auditory. Repeatedly they said that the visual suggestion merely bothered them, made them more uncertain in their reproductions, and increased their inaccuracy. Yet the results scarcely bear them out in this statement. The errors for visual suggestion are rather less in amount if anything than those for auditory suggestion.

At first there was a tendency for the auditory suggestion to arouse an attitude of expectant attention in the mind of the observer. At the command "make high" the observer involuntarily looked for a high triangle to reproduce. After a few trials, however, the conviction was established that there was no connection between the command and the triangle shown, and from that time on all expectancy was eliminated.

B. Vertical Point Distances

In the course of the experimentation with triangles it was frequently felt that the conditions of the experiment were complicated by the fact that the apex was not always in the same lateral position with reference to the centre of the base. A simplification was therefore decided upon, and two sets of experiments were carried out as follows.

1st. Dot above line. A base line 10 cm. long, bisected by a short stroke 2 mm. high, was drawn on each of ten cards. On each card, directly above the bisection, a dot was placed at a distance varying regularly from 2 cm. to 11 cm. from the line. These cards were so numbered from 1 to 10 that the distance from the point to the line in centimeters was always just one greater than the number of the card (*e. g.*, III = 4 cm., IX = 10 cm., etc.).

TABLE III. VERTICAL POINT DISTANCES

No.	C.			D			E.			F.			Aggregate.			Ave.
	St.	Sugg. H	Sugg. L	St.	Sugg. H	Sugg. L	St.	Sugg. H	Sugg. L	St.	Sugg. H	Sugg. L	St.	Sugg. H	Sugg. L	Error of St.
1. Dot above line. Auditory suggestion																
I	+7	+2	−3	+1	+2	−1	+1	+4	+.3	+.3	−.3	+2	+9.3	+7.7	−1.7	2.3
II	−3	−4	−5	+6	+8	+5	+1	−1	−.3	−2	+3	−2	+2	+6	−2.3	3
III	+1	−3	+2	+7	+6	+5	+5	+4	+3	0	+2	+3	+13	+9	+13	3.3
IV	+7	+.3	+4	+3	+4	+1	+3	+7	+6	−4	+.3	+3	+9	+11.6	+14	4.3
V	+14	+14	+7	+2	+2	−.3	+15	+14	+18	−4	+3	+1	+27	+33	+25.7	8.8
VI	+17	+24	+16	+.3	−1	−2	+15	+29	+24	+2	+4	+3	+34.3	+59	+41	8.6
VII	+18	+13	+23	−2	−4	−4	+10	+15	+21	+4	+4	+8	+30	+28	+48	8.5
VIII	+13	+19	+18	+.3	+2	−.6	+20	+27	+19	−1	+1	+3	+32.3	+45	+39.4	9.3
IX	+32	+18	+7	−6	−13	−6	+24	+25	+27	−1	−4	+2	+49	+26	+30	15.8
X	+12	+21	+28	−13	−11	−10	+20	+28	+34	−5	−2	−3	+14	+36	+49	12.5
Agg.	+118	+107.3	+97	−1.4	−9	−12.9	+114	+152	+152	−10.7	+11	+20	+219.9	+261.3	+256.1	
2. Dot above dot. Visual suggestion																
I	−.6	−3	+1	+8	+8	+7	+8	+3	+5	−3	−2	+.6	+12.4	+6	+13.6	4.9
II	+1	+12	+10	+8	+12	+11	+7	+5	+6	−1	+5	+4	+15	+34	+31	4.3
III	−2	+6	+3	+10	+11	+9	+12	+9	+16	−.3	+3	+1	+19.7	+29	+29	6.1
IV	+1.4	+5	+3	+9	+12	+16	+19	+18	+16	−2	+2	0	+27.4	+37	+35	7.9
V	−5	+5	−1	+5	+16	+8	+26	+18	+15	−5	+5	+6	+21	+44	+28	10.3
VI	+7	+6	+2	+10	+2	+5	+22	+29	+31	−8	+2	−3	+31	+39	+35	11.8
VII	−1	+1	+5	+6	0	+2	+22	+23	+20	−6	+3	−.6	+21	+27	+26.4	8.8
VIII	−2	−1	+9	+6	+.6	−4	+30	+31	+30	−7	−1	0	+27	+29.6	+35	11.3
IX	+2	+1	+2	+1	+3	−10	+29	+29	+35	−4	−.3	+3	+28	+33.3	+30	9
X	+10	0	+8	+1	−7	−.6	+18	+29	+38	−7	−8	−7	+22	+14	+38.4	9
Agg.	+10.8	+32	+42	+64	+57.6	+43.4	+193	+194	+212	−43.3	+9.3	+4	+244.5	+292.9	+301.4	

2nd. Dot above dot. On each of a like number of cards two dots were so placed that the conditions were the same as above except that the base lines were lacking.

These cards were observed through the box described above. In the first set the observer was given a sheet of paper with a 10 cm. line on it, bisected as the one on the card, and was asked to place a dot at the same distance above the bisection as the one shown on the card. In the second set the paper carried only a dot instead of the line. The procedure was the same as that for the triangles. Each set was shown to each observer nine times. The first, fourth, and seventh series were given without suggestion and the results for each card were averaged for the standards. The other series were given with high and low suggestion, the cards being taken at random, and now the odd numbers were given with high suggestion the even with low, now *vice versa*. It was intended to give both sets of dots with auditory and visual suggestion, but the work was interrupted when the dot above line set had been completed with auditory suggestion and the dot above dot set with visual. Thus the results are not directly comparable. Only observers C, D, E and F took part in the dot experiments. The average and aggregate errors are given in Table III. An inspection of the table reveals several interesting tendencies.

1. Standards. (1) Aggregates and average error for the different vertical distances. The aggregate error of all observers for each vertical distance is uniformly positive. This uniformity is much more marked than was the case with the triangles. Nor is it due altogether to the absence of results from A and B. This naturally raises the query whether the sides of the triangles, particularly of the higher ones, may not have something to do with the reduction of the error. Perhaps it is a case of overestimation of acute angles such as characterizes the Poggendorff and Zöllner illusions, the overestimation of the acute apical angle in this case causing a flattening in the appearance of the triangle. In some also a relation to the Müller-Lyer figure may be traced. To settle this question the present data are insufficient. It will be noted that both the aggregate errors and the average errors of the standards increase roughly with the increase in vertical distance, though the last distance (11 cm.) does not show the largest error in either set. Further it will be seen that the average error of the standards has a greater range and is more regular in its variation in the first set than in the second.

(2) Errors of individual observers. The same general types regarding the nature of the error in each observer were noted as in the work with triangles, *i. e.*, C, D, and E were positive, F negative. It is true. that in D's case the aggregate error

for all distances in the dot above line set is slightly negative (—1.4 mm.), but this is due to an exceptionally large negative error for the 11 cm. distance. Seven out of the ten distances show a small positive error.

The way in which the error manifests itself in the different observers is striking. C and E begin with a small positive or slightly negative error for the lesser distances in the dot above line and advance to a large positive error (+32 cm.) for the greater distances. E behaves in the same manner with the dot above dot, but C keeps a small error here and fluctuates about the zero point with a slight balance on the positive side. These facts are in accord with the introspection of the observers. Both C and E had a greater feeling of certainty in placing the dot at short distances from the base line. The farther they got from the base line the greater became their uncertainty. With E this was true of the dot above dot also. C, however, found it easier to record the distance between dots than between the dot aud the base line. The base line formed a distraction and the tendency was to outline a whole triangle, comparing the relative lengths of altitude and base, rather than to note merely the distance between the dot and the base line. D showed a small positive error in the shorter and medium distances in both sets, and, in sharp contrast to C and E, tended to go over into a negative error in the greater distances. D found it harder when only the dots were given, for, she said, "when a line was given I could measure the distance from the line to the dot by the length of the line, but when only a dot was given I had no measure." The same tendency, however, to go over into a negative error in the greater distances was observed in both sets. F showed a prevailing negative error in both sets and this tended to increase slightly with the greater distances especially in the dot above dot set. She, like C, felt surer of the distance between the dots than between the dot and line, yet her aggregate error is less for the latter than for the former.

2. Auditory suggestion; dot above line. There is little indication that the suggestion had any considerable effect. With C and D the low suggestion is lower than the standard, but the high suggestion, while higher than the low, is likewise lower than the standard. The result of either suggestion is to lower the error, the low being the more effective. With E and F, on the other hand, the opposite tendency is observed. The high suggestion gives an error higher than the standard, but the low suggestion produces an error just as high or even higher.

3. Visual suggestion; dot above dot. Here D and E maintain the same relative tendencies which they showed in the

auditory series, the former showing standard, high, low in descending order, the latter in ascending. C here changes decidedly to the ascending order, while F goes from an aggregate error of —43.3 for standard to +9.3 for high suggestion and drops back to +4.6 for low suggestion.

Discussion

Without doubt the most potent factor in cases of suggestion is the arousal of an attitude of expectant attention. In the experiments of Small and Binet pains were taken to arouse the highest possible degree of expectancy, and the whole of the illusion is referable to this attitude. In Pearce's experiment the result is due to shifting the attention from one stimulus to another, the reaction representing the resultant of both. If the focal idea is sufficiently dominating, and the situation does not offer too glaring a contradiction, there is almost no limit to be set to the amount of the illusion. In the present experiment expectant attention was gradually eliminated from the consciousness of the observer, and an effort was made to determine to what degree the mere sensory stimulus unwittingly influenced the reproduction. We have seen that with auditory suggestion in the triangles the influence was considerable, with the visual suggestion it was less marked, while with the dots the results were so contradictory as to be difficult of interpretation. This is due not to the changes in the conditions so much as to the time and practice elements. A somewhat similar decrease in the effect of the suggestion after repetition is observable in Binet's final trial. In our own experiments it seems to be a case of the motor power of ideas. In the first set, where the method of procedure was relatively novel, the idea aroused by the suggestion was sufficiently vigorous to influence the placing of the dot. But as the investigation proceeded a twofold process of habituation took place. 1st. The habit of judging of the situation and placing the dot became more and more automatic, less affected by other idea groups. 2nd. The constantly reiterated suggestion, known to have no definite connection with the figure to be reproduced, roused an idea weaker and weaker in its motor power, until it had only a slightly disturbing or distracting effect upon the reaction. The observer thus became habituated to leaving this stimulus out of account.

Like Pearce, Brand, and Smith and Sowton we found striking individual differences in susceptibility to suggestion. Observer B was the most uniformly susceptible, followed closely by A. Observers D and F, on the other hand, show little sign of being affected by the suggestion even from the first.

Mention was made of the fact that Binet, and Smith and

Sowton found uniform and very striking negative errors in reproduction without suggestion, and that of Brand's subjects two were negative and but one positive. In our experiments, on the other hand, four observers are of the positive type and two negative, while the aggregates are strongly positive throughout. It must be remembered, however, that the other investigators employed horizontal distances while ours were all vertical. The difference in results may, therefore, be connected with the well known vertical-horizontal illusion, where if two lines of equal length, the one vertical, the other horizontal, be observed the vertical will appear the longer. Further, Brand found that in general the long suggestion was more effective than the short, whereas with us the low suggestion was more effective than the high. If we take this in connection with the prevailing types of error in the two cases the generalization would seem to be warranted that with the predominantly negative type the suggestion to increase the distance will be more effective, while with the positive type the suggestion to shorten the distance will have the greater efficacy. This is corroborated by an examination of the results for the individual observers in our tables.

Summary.

1. In reproduction without suggestion the aggregate error for all point distances and all triangles except IX and X was positive, *i. e.*, the observers tended to place the dots too high.

2. In the series without suggestion the type of error remained constant for each observer in all sets. All the observers were of the positive type except B and F.

3. There were decided individual differences in susceptibility to suggestion. B was the most susceptible, D and F the least.

4. The suggestions were most effective in the first set of experiments (triangles with auditory suggestion), less so in the later ones.

5. Low suggestion was more effective for the positive type, less effective for the negative type, than high suggestion.

A STUDY OF TEMPERAMENTS AS ILLUSTRATED IN LITERATURE[1]

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I

Literature as a Criterion of Life

In one way, that of the analysis and portrayal of temperament, good literature may be regarded as a fair criterion of conditions in real life; in fact, literature *is* good only when it seems, to discriminating judgments, to present humanity and its relations in a worthy and truthful manner. Matthew Arnold's statement that literature is a criticism of life will usually hold true.

Nevertheless, in the matter of the relative proportions of the various temperaments, the study of fiction and the drama tends to be somewhat misleading. As a usual thing, it is only the "interesting" types that the author cares, or, perhaps, dares, to use. The active, the gay, the romantic, the emotional—concerning these, he may feel sure, the reader ardently desires to be informed; the dull, the apathetic, the morose, the timid, the weak, or the merely ordinary, the author is perfectly aware, appeal to the public but little. These latter characters, then, he uses sparingly, or in an accessory way, as foils for the strong, animated and attractive, or as sources of annoyance to the injured innocent. Villains have always been in favor, provided that they were shrewd and vigorous enough, but the commonplace, well-meaning persons, the flabby and characterless, have been but slightly recognized in literature. This tends to be less and less the case, as humanitarianism increases, and the personality of the individual occupies a larger place in the minds of men. In other words, modern realism introduces into literature many characters which would formerly have been deemed unworthy of consideration. Modern fiction is, in this sense, a better standard of judgment in the matter of temperaments than those phases of character-portrayal which have preceded it. Yet it must be admitted, that while good literature describes and analyzes human characters with accu-

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racy and fidelity, it forms an extremely unsuitable means by which to estimate the proportions in which the various temperaments appear in the civilized world.

II

Ribot's Classification of Characters

This attempt to classify, on the basis of individual differences, some of the well-known characters of fiction is founded rather largely upon Ribot's discussion of normal characters in chapter 12 (Part II) of "The Psychology of the Emotions." Some modification of that scheme has, however, been deemed expedient. It will be remembered by those who are familiar with the chapter in question, that Ribot prepares the way for his classification by excluding from the catalogue of "true characters" the amorphous, or characterless; and the unstable, or vacillating. He then indicates "four degrees, of increasing definiteness and diminishing generality." Within the first of these, which is the most extensive and general (almost purely theoretical, in fact), he makes two great groups of characters: 1. The Active. 2. The Sensitive. After this twofold division, he is forced to admit the necessity for a third, which shall include those characters that are low both in feeling and in the impulses to action. This gives us: 3. The Apathetic. In the second degree come "the fundamental types of character," not merely theoretical, like those in the first degree, "but real, justified and verified by observation." In the third degree are the mixed or composite characters—those made up of elements from the three large groups. Here we find the sensitive-active, the apathetic-active, the apathetic-sensitive, the temperate or balanced (if there are any such, which Ribot appears to doubt). The fourth degree includes the "partial characters" for which we are given the formula: "Amorphousness, *plus* an intellectual disposition, or a well-marked affective tendency." [1]

Without indulging in any extended commentary on this scheme, which in the main seems reasonable, one may be allowed an expression of concurrence with the criticism of it that Malapert makes in his "Les Elements du Caractère." "It [the classification of Ribot] sins by default. It excludes too much." Have we the right to exclude the amorphous and the unstable on the ground that they are not real characters? They have a certain claim, by reason of their very peculiarities.

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[1] This outline does not, of course, pretend to any classification of abnormal, or morbid characters, which are, according to Ribot, to be distinguished by means of his test of "unity and stability," and which he treats at some length in chapter 13.

Belonging to numerous reasonably normal human beings, they must constitute characters of some sort, just as a very ill-developed brain is nevertheless a brain. They seem, psychologically considered, to be as worthy as any other types; for as Malapert very wisely remarks, "Is character more than the moral physiognomy of the individual?"

In this paper, the more important character divisions, indicated by Ribot, have been adhered to, with, however, little effort to preserve a rigid observation of the "four degrees." The catalogue of "real" characters has been enlarged, to contain the amorphous, the unstable, and the temperate, or balanced character which Ribot regards somewhat doubtfully.

III

The Active Temperament

"The active," says Ribot, "have as their dominant characteristic a natural and continually renewed tendency to action. . . . Their life is mostly directed outwards. . . . They are optimists, because they feel strong enough to struggle with obstacles, and overcome them, and take pleasure in the struggle."

We need not search long in literature for examples of the mediocre and higher forms of the active temperament. D'Artagnan in "The Three Musketeers" of Dumas, serves admirably as an illustration of this type. His first adventures at the Court of Louis XIII are startlingly strenuous; the most noteworthy being the encounter with the Cardinal's guards. In this affray, D'Artagnan conducts himself with astonishing audacity and courage, finishing with his own hand an incredible number of experienced swordsmen. After this, duels, skirmishes, and bloody battles follow with appalling rapidity. The young Gascon no more than walks out for his dinner but he feels called upon to thrust his sword through a half-dozen ferocious enemies, of whom the supply never seems to grow less, despite his amazing achievements. There is no time for the hero to think, meditate, develop fine, differentiated emotions; no time for the author to describe them, if they existed. All is action, sword-thrusting, blood-letting, "swashbuckling." The feelings that play upon the surface of this exciting succession of events are the primitive ones—fear, courage, jealousy, hate, ambition, rude affection,—all in their simplest, least complicated form. The prime motive in the hearts of D'Artagnan and his daring companions is clearly the zest of life, the fine fervor of over-abundant spirits, the unbridled love of action, movement, excitement, and adventure.

Of the same enterprising, headlong, life-loving type are all

the heroes, little and big, of the usual historical novel; often, also, of the historical play. Witness Prince Hal, and Hotspur, two vigorous and lusty soldiers, joying in wild life, opposition, struggle, the battlefield. The Prince, who has a generous and very real comprehension of the character of his rival, thus sums up the passionate activity of Harry Percy: "I am not yet of Percy's mind, the Hotspur of the North; he that kills me some six or seven dozen of Scots at a breakfast, washes his hands, and says to his wife, 'Fie upon this quiet life! I want work!' 'O, my sweet Harry,' says she, 'how many hast thou killed to-day?' 'Give my roan horse a drench,' says he, and answers, 'Some fourteen . . . a trifle, a trifle!'" (Henry IV, Part 1, Act II, Scene IV.)

We might go through a long list of such knights of the sword and pistol: Quentin Durward,[1] Barry Lyndon,[2] Front de Boeuf,[3] Ivanhoe,[4] St. Ives,[5] David Balfour,[6] Henry Esmond,[7] Richard Carvel,[8] Hugh Wynne,[9] and a thousand others, as well known, or now fallen into literary obscurity. Some have more of one characteristic, some of another; some are merely roystering dare-devils, some polished diplomatists and gentlemen; but all are first and foremost distinguished by the love of action, the irrepressible desire for novelty and adventure, that leads them into and safely through the most prodigious and perilous of exploits.

The great-active temperament is that of the ordinary active, but more single-minded, more powerful, more exalted. In this class we must put all the epic heroes, and all great leaders of men, and makers of history, as they appear in literature worthy of their prowess. Hector,[10] Achilles[11], Odysseus,[12] Æneas,[13] Roland,[14] Siegfried,[15] Sigurd,[16] Arthur,[17] Beowulf,[18] the Cid,[19] and the Satan of Paradise Lost,[20] are all great actives. Hamilcar,[21] Cæsar,[22] Richard the Lion Heart,[23] William Tell,[24] and other such personal forces, may go in the same category. The great-active, though it may stoop to little things, has its vision on something far beyond the commonplace rewards of life—on the conquering or the restoration of a people, a nation, or a world. It is purposeful, vigorous, dauntless, and often (in the books), superhuman. It may

[1] Quentin Durward: Scott. [2] Barry Lyndon: Thackeray. [3] and [4] Ivanhoe: Scott. [5] St. Ives: R. L. Stevenson. [6] David Balfour: R. L. Stevenson. [7] Henry Esmond: Thackeray. [8] Richard Carvel: Winston Churchill. [9] Hugh Wynne: F. Wier Mitchell.

[10] - [11] The Iliad: Homer. [12] The Odyssey: Homer. [13] The Æneid: Virgil. [14] Chanson de Roland. [15] Nibelungen Lied. [16] Sigurd the Volsung: Wm. Morris. [17] Arthurian Legends. [18] Beowulf (the Anglo-Saxon Epic). [19] The Cid (the Spanish Epic). [20] Paradise Lost: Milton. [21] Salammbô: G. Flaubert. [22] Julius Cæsar: Shakespeare. [23] The Talisman: Scott. [24] Wilhelm Tell: Schiller.

itself be conquered, and even suffer death, but in the largest sense it is never really defeated; it transcends all that mere earthly strength can bring to pass. It is human possibility raised to the n^{th} power, and, in the older literatures, at least, embodies the ideal man approaching the potency and omniscience of the gods.

It will be noted that, so far in the discussion of the active type, only the names of men have been given. Are there, then, no active women, either in literature or in life? Doubtless there are more in real life than in the realms of fiction; yet it would be difficult to imagine a woman of the dominantly active temperament, such as D'Artagnan, or Henry Esmond, is seen to possess. The conventionalities have too firm a hold upon the sex, for one thing, and a women's nature is too inherently emotional, for another. Even though she may, by taste or necessity, be distinctly active, in whatever polite way is permitted, she is bound to be dominated by her feelings. The woman who is the exception to this rule, though she may be a valuable member of society, is not usually attractive and romantic enough to be made the heroine of a novel. She may take the place of a subordinate character, but that is all. Tom Grogan, in the book of that name by Hopkinson Smith, is a woman who leads an active, masculine life—but yet a woman. Priscilla Lammeter[1] and Betsy Trotwood[2] might be spoken of as active women, yet they are consigned to the position of inferior, half-humorous characters in the books in which they appear. The dearth of purely active feminine types in literature is significant of several things: First:—That the emotional or emotionally active women are the only ones really interesting to the average, or even the intelligent reader. Second:— That the usual masculine writer (and up to a few years ago, practically all writers were masculine) sees woman only as an emotional subject, because he regards all women as different, in mental constitution, from his own sex. Third:— That the usual feminine writer sees woman only as an emotional subject, since she knows all or nearly all, women to be like herself. Fourth:— That women are, actually, by birth and training, emotional and not pre-eminently active.

Thus it is easy to see why, though there are plenty of sensitive-active women in literature (as will be shown later), there is an extremely small number in whom activity may be said to dominate.

IV

The Sensitive Temperament

Within the genus of the sensitive, Ribot distinguishes three species: The humble; the contemplative; the emotional.

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[1] Silas Marner: G. Eliot. [2] David Copperfield: C. Dickens.

The first of these, the humble, is characterized by "excessive sensibility, moderate intelligence, and little or no energy." Such a personality is rarely found in fiction; it is difficult to handle, and does not attract the author whose chief purpose is to please his readers. It is to the work of a Balzac that we instinctively turn, to find an example of such a character as the sensitive-humble, disregarded by the generality of literary men. "Cousin Pons" meets the requirements precisely. Pons is a bachelor, somewhat advanced in years, a musician and a collector of rare and beautiful objects, for which he spends the greater part of his scanty income. He has, the author tells us, "a great soul, a sensitive nature."

"The sense of beauty which he had kept pure and living in his inmost soul, was the spring from which the delicate, graceful and ingenious music flowed, and won him reputation." "A masterpiece finely rendered, brought tears to his eyes."

This "tender, dreamy and sensitive soul" is condemned to loneliness. The few relatives whom Pons can claim are stolid or malicious creatures, incapable of any understanding of the gentle bachelor's nature. "He had," says Balzac, "suffered acutely among them, but like all timid creatures, he kept silence as to his pain; and so by degrees, schooled himself to hide his feelings, and learned to take sanctuary in his inmost self." "Who will ever paint all that the timid suffer?"

The wretchedness which even trifles could cause him is compared to the irritation of some harsh substance on a delicate membrane. "Invisible grains of sand sank perpetually into the fibres of (his) being, causing . . . intolerable anguish of heart."

On the occasion of a visit paid by the old man to his stupid and mercenary cousin, "Pons suffered from the inexplicable emotions which torment clear consciences—from a panic terror such as the worst of scoundrels might feel at sight of a policeman; an agony caused sorely by the doubt as to Madame de Marville's probable reception of him." At some carelessly brutal remark from the same cousin, "Pons flushed red, like a girl found in a fault. The grain of sand was a little too large; for some moments he could only let it work in his heart."

Here is the perfection of the sensitive-humble type; we need seek no better illustration than this shrinking, palpitating, self-distrusting old soul, so exquisitely depicted by the hand of him who has been successful above all others, in the delineation of elderly men.

Pére Goriot, another of Balzac's characters, is very similar to Cousin Pons. He is retiring, self-sacrificing, delicately impressionable, a victim of his own generosity and the selfishness of others. Neglected and ill-treated by his daughters, to

whom he has given all, he represents in his lonely and pitiable death the tragedy of the sensitive-humble character in an unregardful world.

Brutus[1] is an excellent example of the sensitive-contemplative character, though perhaps not so striking as the much-discussed and never perfectly analyzed Hamlet. Brutus is always the thinker, the man of speculative imagination; yet his thoughts and imaginings are always tinged with feeling—with sincere affection for his relatives and friends, with kindliness for servants and attendants, with passion of noble action or fervor of patriotism. He does nothing in the rashness of personal enmity, nor is he moved to outbursts of wrath and spleen, as Cassius is. He ponders all his deeds, weighs every consequence, and considers every possibility. When he acts, it is only after long inward discussion and the wearisome comparison of right and wrong. He never strikes out boldly, as the man of action does; nor, on the other hand, does he shrink from a strenuous duty, as the mere emotional weakling is prone to do. Though moved by strong emotional impulses, he debates long within himself, as to a proper course of action and having decided what is right, holds rigidly to it, to the end.

The Buddha of Edwin Arnold's "Light of Asia" is a most satisfying example of the sensitive-contemplative type. Gentle, affectionate, full of love and ardor for his family and friends, he yet withdraws from the world into a life of meditation,

> "Seeking, night and day,
> * * * that light which somewhere shines,
> To lighten all men's darkness, if they knew:"

Lost in contemplation he passes to vision after vision of

> "Life's upward steps, long linked
> To higher slopes and higher,"

The man rapt in his holy thoughts becomes at last the mystic and the seer.

Other illustrations of the type may be found in literature of greater or lesser worth. Hamlet has already been mentioned. Job never ceases to fascinate, with his long and well-nigh unendurable afflictions, his restless searching after truth, his wistful, half-skeptical questioning, "If a man die shall he live again?" Christian in the "Pilgrim's Progress" is a sensitive contemplative; so is Silas Marner in George Eliot's great novel; and in the same list with these we shall be justified in putting Dr. Primrose,[2] Robert Elsmere,[3] Dr. Lavendar,[4] and John Ward, Preacher.[5]

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[1]Julius Cæsar: Shakespeare.
[2]The Vicar of Wakefield: Goldsmith. [3]Robert Elsmere: Mrs. Humphrey Ward. [4]Old Chester Tales, Dr. Lavendar's People: Margaret Deland. [5]John Ward, Preacher: Margaret Deland.

The sensitive-emotional temperament finds a fitting embodiment in the Werther of Goethe.[1] Werther is above all things the man of feeling. He seems almost incapable of action, so much are his mental powers concentrated on his own states of emotion. He appears to be endowed with talents, particularly the artistic, of which, in his letters, he occasionally speaks. Even before he is permitted that first fatal view of Charlotte cutting bread and butter for her flock of brothers and sisters, he is seen to be highly emotional, and imaginative to an extreme degree. He confesses that he has ever been the subject of sudden transitions from sorrow to immoderate joy, and from sweet melancholy to violent passions. After he becomes possessed by that unhappy affection which works his utter undoing, we behold him with eyes in a fine frenzy rolling, carried away by the violence of his love.

On one occasion, he tells us, overcome by a force of a purely imaginary circumstance, he buries his face in his handkerchief, and hastens from the room.

"If Charlotte," he says, "does not allow me to enjoy the melancholy consolation of bathing her hands in my tears, I feel compelled to tear myself from her. Then I either wander through the country, climb some precipitous cliff, or force a path through the trackless thicket, where I am lacerated and torn by briars, and thence find relief."

Again, in writing to his friend Wilhelm: "O, why cannot I fall on your neck, and with floods of tears and raptures give utterance to all the passions that distract my heart?" He relates an instance in which Charlotte figures: "I threw myself at her feet, and seizing her hand, bedewed it with a thousand tears." He becomes a pessimist, as those who feel deeply must inevitably do. "What is the destiny of man," he questions, "but to fill up the measure of his sufferings, and to drink his alloted cup of bitterness?" Yet he of all the world appears to himself the most miserable. "Have men before me ever been so wretched?" he exclaims. And so on, through fits and gusts of passion, wails and ravings of despair, we follow him to the end, when he takes his wretched life with the pistols which his last letter tells us he has kissed a thousand times, because the white hand of Charlotte has but touched them.

It is said that Werther has been responsible for many suicides in addition to his own. This is easy to believe. In the sentimental age in which the book was written, there may, indeed, have been many who, impressed by the fervor and eloquence of the passionate young artist, were persuaded that they, too, had griefs too bitter to be borne. But in this matter-

[1]The Sorrows of Werther: Goethe.

of-fact age there are few who can read "The Sorrows of Werther" without a certain impatience, even mirth. He is the emotional hero *par excellence*, but the time has gone by when such heroes can make their appeal.

Of the sensitive type but more attractive and lifelike is Lucy Snow, in Charlotte Brontë's delightful novel, "Villette." Lucy is frankly emotional, yet full of intelligence, and, usually, of self-control. She has little impulse to action, and never takes an initiative unless forced by necessity to do so; she rather remains passive, feeling intensely, yet doing nothing, until a desperate situation of some sort compels her to take a new course. She suffers much, not particularly from ill treatment, or even unkindness, of which she has an undue share, but from the sensitiveness of her own soul, which torments itself with questions, reproaches, vague imaginings, and passionate desires. To the looker-on, a small, plain, quiet English teacher in a busy and pretentious Belgian *pensionnat*, she is in reality the central figure in a long series of emotional dramas—all enacted within the chambers of her own brain. She has a highly wrought, keenly susceptible nature, yet, lacking neither spirit nor humor, she never seems exaggerated nor artificial. She is, indeed, one of the most natural, as well as one of the most skillfully analyzed emotional figures in all modern literature.

Jane Eyre[1] is much like Lucy Snow, though perhaps somewhat more intense. There is a long list of sensitive-emotionals in fiction and the drama. Corinne[2] is noteworthy; so is Clarissa Harlowe[3]—a figure of almost pure emotion. Juliet[4] is all ardor and sensibility—a most fitting companion for the youthful Romeo; Ophelia,[5] Desdemona,[6] Miranda[7] and Viola,[8] not to mention others of Shakespeare's women, may well be said to come within the catalogue of emotional. Cyrano de Bergerac,[9] Heathcliff,[10] Rochester,[11] Arthur Dimmesdale,[12] Claude Melnotte,[13] though it may sound incongruous to group them together, are sensitive-emotional men. Nydia,[14] Evelina,[15] Pamela,[16] Tess of the D'Urbervilles,[17] Jeanie Deans,[18] Hester Prynne,[19] Lindel of the "African Farm,"[20] and the emotional heroines of the so-called "love stories" are of this

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[1] Jane Eyre: C. Brontë. [2] Corinne: Mme. de Staël. [3] Clarissa Harlowe: Richardson. [4] Romeo and Juliet: Shakespeare. [5] Hamlet: Shakespeare. [6] Othello: Shakespeare. [7] The Tempest: Shakespeare. [8] Twelfth Night: Shakespeare. [9] Cyrano de Bergerac: Rostand. [10] Wuthering Heights: Emily Brontë. [11] Jane Eyre: C. Brontë. [12] The Scarlet Letter: Hawthorne. [13] The Lady of Lyons: Bulwer-Lytton. [14] The Last Days of Pompeii: Bulwer-Lytton. [15] Evelina: Miss Burney. [16] Pamela: Richardson. [17] Tess of the D'Urbervilles: Thomas Hardy. [18] The Heart of Midlothian: Scott. [19] The Scarlet Letter: Hawthorne. [20] The Story of an African Farm: Olive Schreiner.

type, as well as many in books of higher excellence. It may truly be said that the common feminine type in literature is the sensitive-emotional as the common masculine type is the active.

V

The Apathetic, the Amorphous, and the Unstable

Of the really apathetic character we see but little in literature, chiefly, perhaps, because it does not make interesting reading. It is apparent, nevertheless, in a few well-marked cases. One of the most typical of these is Minoret-Levrault in Balzac's "Ursule Mirouet." He is a heavy, repulsive person, lacking in both activity and emotion. His wife, Zèlie (an apathetic-active, it would seem), completely dominates him, scheming and plotting for the advancement of the family, and managing everything according to her own very narrow and very selfish ideas.

"Wherever form rules," says the author, "sentiment disappears. The postmaster, a living proof of that axiom, presented a physiognomy in which an observer could with difficulty trace beneath the vivid carnation of its coarsely developed flesh, the semblance of a soul, . . . Though . . . quite incapable of reflection, the man had never done anything that justified the sinister suggestions of his bodily presence. To all who felt afraid of him, his postilions would say, 'Oh, he's not bad.' He seldom spoke. . . . If he had been a talker you would have felt that he was out of keeping with himself. Reflecting that this elephant minus a trumpet, and without a mind was called Minoret-Levrault, we are compelled to agree with Sterne as to the occult power of names, which sometimes ridicule, and sometimes foretell character."

It is needless to say that the sluggish and unprepossessing postmaster is not the most interesting character in the book.

On the whole, the pure-apathetic type must of necessity carry with it a suggestion of stupidity. There is something abnormal, also, about it, for it is the nature of the normal man either to feel or do—or to both feel and do. Usually, in literature, the apathetic person has some moving impulse within him, which puts him into the list of apathetic-actives, or that of partial characters. Bovary, for instance, in Flaubert's great novel, "Madame Bovary," would be set down as an apathetic were it not that his simple, but deep and intense love for the unworthy woman he has married gives him a claim to a somewhat higher place. Dunstan Cass, in George Eliot's "Silas Marner," is of an apathetic temperament, yet once or twice he acts with vigor and decision. Dombey, in Dickens's "Dombey and Son," approaches the apathetic, but exhibits on certain

occasions an ability to feel and do that makes us hesitate in our classification of him. The apathetic temperament in the normal man is, it would seem, best distinguished in combination with elements of a distinctly different sort.

The amorphous characters approach the sensitive-emotional on the one hand and the apathetic on the other; ordinarily there may be some difficulty in distinguishing them. A notable example is, however, ready at hand, which represents rather clearly what Ribot means when he says of the amorphous, "Some other person, or failing that, the social environment, wills for them, and acts through them. They are not voices, but echoes." This character is seen in Gertrude, Queen of Denmark, the mother of Hamlet. Her first utterance in Shakespeare's play is a commonplace, a cheap platitude, acceptable only to shallow minds:

"Do not forever with thy vailèd lids
Seek for thy noble father in the dust;
Thou knowst 't is common; all that lives must die,
Passing through nature to eternity."

"Ay, Madam, it is common," answers Hamlet, sensible for the moment that to try to explain real and lasting grief to a nature like the queen's is a useless and thankless task.

"If it be," continues his mother, "why seems it so particular with thee?"

Hamlet's patience gives way. Here is a woman whose husband is less than two months dead, who is able to console herself, and now attempts to console him with a meaningless formula caught from other lips, perhaps those of the "borrowed majesty of Denmark." The young man bursts forth into passionate reproach:

"Seems, Madam! nay it *is!* I know not seems!"

But the truth and eloquence of his speech are lost upon the shallow queen. The remainder of the play goes to show her almost utter lack of individuality. When Hamlet, in the closet scene, attempts to show her the heartlessness and iniquity of her conduct, she seems at first unable to comprehend his point of view, but soon, with characteristic adaptability, acquiesces completely in what he says, suiting her attitude to his words, and promising anything he asks. No sooner, however, is she out of the circle of his influence than she is again the easy tool of the king, and the reflection of the conventionality and corruption of the court.

She is not the apathetic type, for she has feelings, such as they are, of a rather fleeting nature; she seems to care sincerely for Hamlet, and shows real affection for Ophelia. She is not the unstable, exactly, for she is not within herself changeable or capricious; she is altered by the influence of others, of

more powerful personality than herself. She is not the sensitive-emotional, because her feelings are never deep enough nor strong enough. There seems no doubt as to the justice of classifying her as almost purely amorphous.

The following characters may be classed as amorphous though some of them, it must be confessed, might as reasonably be called partial characters: Genevra Fanshawe,[1] Amelia Sedley,[2] Cosette,[3] Marius,[4] Ursule Mirouet,[5] Stephen Guest,[6] Agnes Wickfield,[7] Hetty Sorrel,[8] Lucie Manette,[9] Lucy Deane.[10]

Capriciousness, uncertainty, lack of poise are characteristic of the unstables. There is no predicting what they will do. They surprise us with sudden changes—unexpected outbursts of malice, kindliness, devotion or self-sacrifice. They add an element of speculation to a story in the very fact that we cannot depend upon them from one moment to another. Nevertheless, they are often irritating because they allow us no settled attitude or verified opinion regarding them. The unstable character as a hero is likely to prove a very unsatisfactory being. Of the truth of this statement we need no further evidence than Mr. Barrie's "Sentimental Tommy," with its tantalizing and disagreeable sequel, "Tommy and Grizel." As a child, Tommy is undeniably attractive, as we expect but little of an infant, or even of a half-grown boy, in the way of stability and unity of character. But as a man, he is not much less than repellant. His explosive, fickle, unreliable character makes him almost as intolerable in fiction as a person of his nature would be in an actual social circle. More successful, because not so completely variable is the character of Sidney Carton, in Dickens's "Tale of Two Cities." His one supreme act of renunciation, surrounded as it is by the pathos of unrequited love, wins for Carton our sympathy and lasting approbation, setting him somewhat above the fairly conventional hero, Evremond. Steerforth, in "David Copperfield," is of an unstable temperament. So, most decidedly, is Peer Gynt, in Ibsen's drama of that name. Godfrey Cass[11] and Arthur Donnithorne[12] are examples among George Eliot's characters. King Lear, though one of Shakespeare's greatest creations, is, nevertheless, of the unstable type. "He hath ever but slenderly known himself," says his daughter, with her cool, unlenient judgment. "The best and soundest of his time hath been but rash." The inconstant character in fiction as in real life, encounters everywhere sorrow and disaster. His condemnation is that of

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[1]Villette: C. Brontë. [2]Vanity Fair: Thackeray. [3]-[4]Les Miserables: Hugo. [5]Ursule Mirouet: Balzac. [6]The Mill on the Floss: G. Eliot. [7]David Copperfield: Dickens. [8]Adam Bede: G. Eliot. [9]A Tale of Two Cities: Dickens. [10]The Mill on the Floss: G. Eliot.
[11]Silas Marner: G. Eliot. [12]Adam Bede: G. Eliot.

Reuben,[1] who heard among the lofty phases of his father's blessing, the sad, inevitable words, "Unstable as water, thou shalt not excel."

VI

Composite Characters

The apathetic-actives are closely allied to the apathetic-intellectuals, and it is difficult to make any clear distinction between them. The intellectuals, however, seem to lack a certain spontaneity, or inner source of action, which the apathetic-actives possess. The chief difference between these active temperaments and the ordinary active, is that the former have less than the ordinary amount of feeling. They are "cold-blooded" as we say. They have one strong passion, as selfishness, or religious fanaticism, but, otherwise, are not responsive to emotional appeals. There is no kindliness, tenderness, nor pity in them; they are unable to put themselves, mentally, in the place of another who is experiencing joy or pain. They make others suffer, even administer torture, without any qualms, either æsthetic or conscientious. An illustration of this temperament appears in the Richelieu of De Vigny's "Cinq-Mars" and Bulwer-Lytton's "Richelieu." He is cold, calculating, impervious to any appeal which touches the usual man. He possesses a brilliant intellect, which he uses to promote his own cruel and fanatic purposes. He allows nothing to interfere with his own designs, but sacrifices the young, the promising, the innocent,—any one whom he deems capable of putting a stone in his way. We recoil in horror from such a soul. It seems, and justly, to be nothing less than hideous, not entirely human but fiendish.

Much the same character is shown in Iago, in Shakespeare's play "Othello." He seems devoid of feeling, except for a certain tinge of ambition and jealousy. He is cold-blooded, consciencelees, but indefatigable in his labor to prosper his evil cause—in short, he is an apathetic-active.

Others who partially or completely possess this unenviable temperament are Richard III,[2] Goneril and Regan,[3] Becky Sharp,[4] Tito Melema,[5] Madame LaFarge,[6] Madame Thenardier,[7] Roger Chillingworth,[8] Javert,[9] and Fagin.[10] Certainly a much longer list might be made. In many, a close inspection would reveal a near approach to the morbid and unnatural;

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[1]Genesis, 49, 4.
[2]Richard III: Shakespeare. [3]King Lear: Shakespeare. [4]Vanity Fair: Thackeray. [5]Romola: G. Eliot. [6]A Tale of Two Cities: Dickens. [7]Les Miserables: Hugo. [8]The Scarlet Letter: Hawthorne. [9]Les Miserables: Hugo. [10]Oliver Twist: Dickens.

many at the best would have to be set down as partial characters.

By far the most usual, as well as the most interesting character-combination in literature is the sensitive-active. So large a representation has this temperament that one must select merely at random.

Anna Karénina in Tolstoi's novel which is named for her, is not less interesting than others of her kind. The emotional heroine of an emotional novel, she, nevertheless, impresses one as a strong, vital, active character. Our first glimpse of her convinces us of her "dignified vivacity," as the author puts it. "There seemed to be in her person," he goes on, "such a superfluity of life that in spite of her will, it expressed itself now in the lightning of her eyes, now in her smile." We feel her energy and capability at once, in the way in which she adjusts the domestic difficulties of her brother, Stepan Oblonsky, and his wife. Everything that she does bears the impress of a vigorous and unwearying nature. She delights in action as a thoroughbred race-horse delights in it. Yet she is full of emotional ardor and sensibility which suffuse her action, and give her the most undeniable womanly charm. The less sensitive, the happier she might have been—the more contented with her unromantic husband, and less susceptible to the charms of the distinguished Vronsky. She suffers agonies of jealousy, of affection, remorse, and despair. She finds her life as she has made it for herself, unendurable; but even in seeking death, she is actuated, not so much by her wild yearning for relief from pain as by the logical conviction that no other course is open, and by a sort of desperate courage concentrating in one strong and decisive moment the tempestuous vigor and passion of her life.

As has been suggested, there are many sensitive-actives. An extra dash of sensitiveness, if the character is a woman, a trifle more of activity, if the character is a man, will produce the proportion which impresses most of us as fitting, proper and true to life. The struggles and unhappiness of Maggie Tulliver[1] draw largely upon our sympathy, for she seems to us a real person, burdened with actual griefs, and performing actual deeds. Katusha in Tolstoi's tremendous book "Resurrection" holds us in a still stronger way. Lady Macbeth,[2] Antigone,[3] Alcestis,[4] are characters capable of deep feeling and great action. Œdipus,[5] Savonarola,[6] Macbeth,[7] are exalted names of men who could both feel and do. The list is long:

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[1]The Mill on the Floss: G. Eliot. [2]Macbeth: Shakespeare. [3]Antigone: Sophocles. [4]Alcestis: Euripides. [5]Œdipus Rex: Œdipus at Colonus: Sophocles. [6]Romola: G. Eliot. [7]Macbeth: Shakespeare.

Lorna Doone,[1] Robert Falconer,[2] Richard Feveril,[3] Jean Valjean,[4] Romola,[5] Rebecca,[6] Ramona,[7] M. Paul Emanuel,[8] David Copperfield,[9] Edgar,[10] Rosalind,[11] Dinah Morris[12]—and many others whose names it would take long to set down. Some of these approach the balanced temperament, others are more purely emotional, yet all are undoubtedly sensitive-actives.

Of the existence of the balanced character, Ribot confesses himself uncertain. He suggests that if there is a person in whom feeling, thought, and action are nearly or quite equal, this condition would result in suppresion of character, *i. e.*, of any marks of individuality. It is difficult to admit that such need be the case. It would seem that character *per se* must still continue; for is it, as Malapert suggests, anything more than the "moral physiognomy" of a given human being?

Whether or not persons of the balanced character exist in real life, there are plenty of them to be found in books. The one that most readily occurs to us is Sir Charles Grandison in Richardson's famous volumes of that name. The preface written by the author sets forth quite clearly his own modest intention, and the prospective character of his hero.

He "now presents (he says) in Sir Charles Grandison, the example of a man acting uniformly well through a variety of trying scenes, because all his actions are regulated by one steady principle. A man of religion and virtue, of liveliness and spirit, accomplished and agreeable, happy in himself and a blessing to others." Mark the perfect man!

From the outset, what may we expect of such a hero but the most irreproachable manners, the most impeccable virtue, the highest, the most unassailable honor—and in general, the most correct and tiresome behavior? These indeed, are exactly what we find, Sir Charles is so flawless, so "icily regular" so "splendidly null" that the first three volumes would seem sufficient to establish his claims to canonization, without the need of the other three or four. It takes him a long time, however, to condescend to bestow his evenly pulsing heart upon the most angelic, yet palpably unworthy, Miss Harriet Byron. In the meantime this periwigged, lace-trimmed, silk-stockinged paragon conducts himself in a manner quite in accordance with the author's preface. On one of the early pages of the book, he coolly disposes (for the time, at least) of the

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[1]Lorna Doone: Blackmore. [2]Robert Falconer: G. Macdonald. [3]The Ordeal of Richard Feveril: G. Meredith. [4]Les Miserables: Hugo. [5]Romola: G. Eliot. [6]Ivanhoe: Scott. [7]Ramona: Jackson. [8]Villette: C. Brontë. [9]David Copperfield: Dickens. [10]King Lear: Shakespeare. [11]As You Like It: Shakespeare. [12]Adam Bede: G. Eliot.

arrant villian, Sir Hargrave Polfexen, who is summarily abducting the heroine with a view to forcing her into marriage. Sir Charles, without descending from his customary dignity, stops the horses, vanquishes the villain, and rescues the lady. This he does with a delicious nonchalance. "I had not drawn my sword," he remarks, in relating this little adventure to an adoring circle of femininity; "I hope I shall never be provoked to do it in a private quarrel. I should not, however, have scrupled to draw it on such an occasion as this, had there been absolute necessity for it.—Vice is the greatest coward in the world when it knows it will be resolutely opposed; and what have good men engaged in a right cause to fear?"

No wonder, after such an account and such a homily that the listeners exclaim with unreserved amazement, "What an admirable man is Sir Charles Grandison—thus thinking, thus acting!" But why follow him through the remainder of this long apotheosis? We should seek in vain if we expected aught of him but the most consistent and thoroughgoing perfection. It is needless to say that we should conclude him to be (long before we reached the last page) the most unmitigated prig.

Although he would not scruple to draw his sword when there was absolute necessity for it, he differs from the man of action in his very conscientious hesitancy; he differs from the emotional man, in his complete and natural control over his feelings, of which the author constantly assures us he has an abundant supply. In short, his faculties are so nicely adjusted that he can be called nothing else than the balanced character of which Ribot is so doubtful, and which in its masculine form, at least, we seldom or never, meet in real life.

Daniel Deronda[1] and John Halifax[2] may perhaps be intended for balanced characters, but neither of them is particularly attractive. Adam Bede[3] might come under this head also, but he has a certain unpretentious solidity about him which prevents our becoming impatient with him.

Portia[4] is one of the most perfectly balanced feminine characters in literature yet she does not appear tiresome nor impossible as Grandison and Deronda do. Perhaps this is because it does not seem unnatural for a woman to be perfect! Adapting Richardson's description of Sir Charles, we find it fairly applies to Portia: "A woman of religion and virtue; of liveliness and spirit; accomplished and agreeable; happy in herself and a blessing to others."

Lack of space forbids the discussion of the partial characters

1 Daniel Deronda: G. Eliot. 2 John Halifax, Gentleman: D. M. Craik. 3 Adam Bede: G. Eliot. 4 The Merchant of Venice, Shakespeare.

of literature. Many that might be so named would, also, it is safe to say, go as easily into one of the groups of composite characters, according to the predominating element which exhibited itself. A considerable difference of opinion might well be aroused as to the final classification of such partial or composite characters as George Meredith's "Egoist,"[1] or Ibsen's Rebecca West,[2] though such a division of opinion would be neither unnatural nor uninstructive.

As to the abnormal characters in literature, like Falstaff,[3] Don Quixote,[4] Quilp,[5] and others, a volume might be written. Indeed, the task of "pigeon-holing" according to temperaments even the more famous personages in literature becomes upon contemplation, a task so large as to seem little short of presumptuous. The present paper can claim to be nothing more than tentative and suggestive. Much remains to be done, which only the widest possible knowledge of literature and psychology could hope to render at all complete.

[1]The Egoist: G. Meredith. [2]Rosmersholm: Ibsen. [3]King Henry IV; The Merry Wives of Windsor: Shakespeare. [4]Don Quixote: Cervantes. [5]Old Curiosity Shop: Dickens.

THE EFFECT OF VARIOUS KINDS OF ARTIFICIAL ILLUMINATION UPON COLORED SURFACES

By H. E. Houston and W. W. Washburn

It is a well known fact, even to the casual observer, that colored surfaces appear of a different color under artificial illumination. Especially is this true of certain blues and greens, which appear as greens and blues when illuminated by the oil lamp.

The object of the present investigation is to measure the change in color which takes place when various colored surfaces are illuminated by the various kinds of artificial illumination now in common use: *e. g.*, oil lamp, gas jet, candle, incandescent and arc electric lamps, and Welsbach burner: all of which are compared with daylight.

The colors to be measured were compared by means of color-wheel mixtures on two concentric color-wheels. A portion of each wheel was illuminated by the lights in question, an area of 2cm. square being blocked off by means of a screen, and diffused light being excluded by means of diaphragms. All kinds of artificial illumination were made equal in intensity to that of daylight, which was reflected from an open window by means of a heliostat. The intensities were made equal by varying the intensity of the artificial light to be compared. This was done by varying the distance of the light from the illuminated surface. An even illumination over the entire colored surface was obtained by sending both lights through ground glass. The intensity, thus obtained, was approximately that of one thousand candles at the distance of one metre. No other intensity of illumination was used.

The colored discs used on the color-wheel were prepared in the following way. Six pigments, viz., English Vermilion, Mineral Orange, Chrome Yellow, Emerald Green (Paris Green), and Artificial Ultramarine, were mixed in gum-arabic and applied to the discs by means of a brush. After this, the paper while yet wet was dusted with the paint powder. We thus obtained very good colors. These colors were then defined by comparing with spectral lines in the following way. A spectroscope was used, in which all but one color was cut off by means of a screen in the observing telescope; we thus avoided contrast effects, and isolated one color line for comparison. This color would change as the observing telescope was moved and the color thus projected could easily be compared with the colored paper held in the hand. Three com-

parisons were made in the case of each color, the averages and average variations being calculated. This must be regarded as only a rough test of hue, and is not meant to refer to the actual light-composition of the stimuli used. A photometric determination of the intensity of these colored discs is given in one of the tables, the method of measurement being that of the flicker photometer. The intensities of illumination were varied by varying the distances of the light from the rotating discs. The standard white of the flicker photometer was then compared in the same way with the intensity of magnesium oxide, and finally all results were reduced to this standard.

The following tables give the value of six colors illuminated by daylight of which the definition is stated in terms of the same color illuminated by artificial light. Thus by substitution it would be possible to find the value of any color by artificial illumination in terms of the same color by daylight with certain additions.

The tables give the averages and average variations, ten measurements being made in each case. Following the tables will be found a diagram showing in a graphic way the changes in wave length of each of the standard colors caused by the use of artificial lights. The average variations are given in parenthesis, and when only two discs were used (the variation being symmetrical) but one variation is given.

Incandescent			Arc
85.7 R + 14.3 B	(1.3)	=	R
R	(2.0)	=	1.92 Orange + 98.08 R
5.72 R + 94.28 Orange	(.76)	=	Orange
Orange	(1.08)	=	.99Y + 99.01 Orange
52.4 G + 47.6 Y	(.1)	=	Y
Y	(.1)	=	32.4 Orange + 67.6 Y
44.0 B + 56.0 G	(1.8 B)	=	63.9 G + 36.1 Bk. (1.3 G)
66.37 G + 1.94 Y + 31.69 Bk. (1.07G) (1.2Y) (0.8 Bk.)		=	G
59.46 B + 40.54 G	(1.44)	=	31 B + 69 Bk.
96 B + 4 W	(.21 B)	=	16.89 B + 3.09 R + 80.02 Bk. (.12 R) (.24 Bk.)

Incandescent			Oil
91.8 R + 8.2 B	(6.2)	=	R
R	(.9)	=	90.1 R + 9.9 Orange

78.3 Orange + 21. 7 R	(.9) = Orange
Orange	(.8) = 95.72 Orange + 4.28 Y
77.4 Y + 22.6 Orange	(.8) = Y
Y	(1.1) = 82.1 Y + 17.9 G
49.9 Y + 50.1 G	(.65) = G
G	(.8) = 73.0 G + 27.0 B
98.96 B + 1.04 R	(1.8) = B
B	(1.4) = 98.98 B + 1.02 G
INCANDESCENT	GAS
99.6 R + 0.4 B	(.13) = R
R	(.22) = 99.5 R + 0.5 Orange
99.5 Orange + 0.5 Y	(.27) = Orange
Orange	(.33) = 99.6 Orange + 0.4 R
94 Y + 6 G	(.15) = Y
Y	(.7) = 95 Y + 5 Orange
91 G + 9 B	(.8) = G
G	(.12) = 98.1 G + 1.9 Yellow
98.1 B + 1.9 G	(.18) = B
B	(.17) = 99.4 B + 0.6 R
INCANDESCENT	DAYLIGHT
84 R + 16 White	(1.2) = R
24 White + 76 Orange	(.7) = Orange
43 Y + 57 G	(.9) = Y
Y	(.8) = 36 Y + 64 Orange
45 G + 55 B	(1.5) = (.6) 4 G + 96 Black
G	(.6) = 91 G + 9 Y
95 B + 4 G + 1 W	= 13 B + 87 Bk.
(.4) (.1) (0.8)	= (.6)
B	= 49.2 B + 1.6 R + 49.2 Black
	(1.6) (1.1) (1.56)
INCANDESCENT	CANDLE
99.2 R + 0.8 Orange	(.28) = R
R	(.29) = 99.3 R + 0.7 B
99.1 Orange + 0.9 R	(.31) = Orange
Orange	(.60) = 97.7 Orange + 2.3 Y

83 Y + 17 Orange	(.16) = Y
Y	(1.1) = 20 G + 80 Y
97.9 G + 2.1 Y	(.52) = G
G	(.73) = 97.4 G + 2.6 B
98.0 B + 2.0 R	(.64) = B
B	(.46) = 98.1 B + 1.9 G
INCANDESCENT	WELSBACH
55 R + 45 Bk.	(1.1) = R
R	= 54 Orange + 7 Y + 39 Bk (1.2) (0.5) (1.1)
54 Orange + 46 R	(2.0) = (1.5) 53 Orange + 47 Bk.
Orange	(.10) = 47 Orange + 53 Y
62 Y + 38 Bk	(1.0) = (1.1) 54 Y + 46 G
36 Y + 64 G	(1.3) = 50 G + 47 Bk + 3 White (1.1) (1.1) (.6)
G	= 69 G + 6 Y + 25 Bk. (1.2) (.8) (1.3)
94 B + 6 G	(0.8) = (1.5) 38 B + 62 Bk
B	(0.5) = (.5) 93 B + 3 R + 4 Bk. (.2)
84 B + 16 G	(0.4) = B

Intensity of standards, in terms of standard white used in experiments and of oxide of magnesium :—

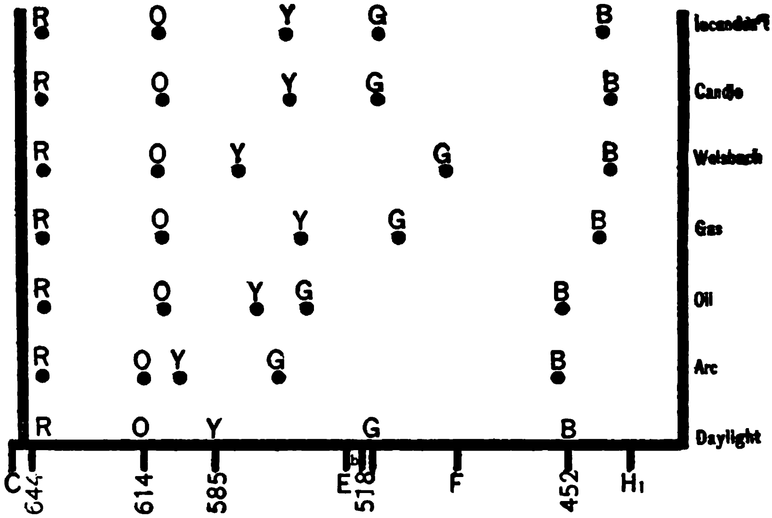
Color	Red	Orange	Yellow	Green	Blue	Black	White
Wave-length	616	604	546	535	447		
Intensity in terms of standard white	28.9	32.0	56.7	17.8	2.9		
Average variation	1.14	2.00	1.56	1.44	0.50		
Intensity in terms of magnesium oxide	26.7	29.4	52.2	16.4	2.7	8.2	92.0 + 1.3

Color-wheel definition of white under arc lamp illumination:—Gray (white) = 16 White + 84 Bk = 14.1 R + 66.1 G + 19.8 B.

CHECK UPON PHOTOMETRY.

Intensity of R + G + B = 26.4 %
" " Bk. + White = 23.5 %

Difference in intensity = 2.9 %


R O Y G B
Candle
Welsbach
Gas
Oil
Arc
Daylight
C
644
614
585
E
b
518
F
452
H1

A BIBLIOGRAPHY OF THE SCIENTIFIC WRITINGS OF WILHELM WUNDT

By E. B. TITCHENER and L. R. GEISSLER

The following list is printed as the beginning of a bibliography of Professor Wundt's scientific and philosophical writings. We hope to continue it in annual installments (the first supplementary list to appear in October, 1909), and we shall be grateful for any corrections and additional items that the readers of the *Journal* may be able to supply.

More than five-sixths of the titles here given have been verified by the one or the other of the compilers. Most of the remainder have been looked up by correspondents; a few have been taken from trade-catalogues and existing bibliographies.[1]

Editor

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[1] Our thanks are due, in particular, to the Librarian of the Surgeon General's Office, Washington; to Dr. L. N. Wilson, Librarian of Clark University; to Professor R. M. Yerkes and Dr. B. Rand, of Harvard University; to Mr. C. E. Ferree, of Bryn Mawr College; and to Mr. W. H. Austen, Reference Librarian of Cornell University.

niss seiner sämtlichen Schriften. [Herausgegeben von W. Wundt.] 2 vols. 1907. Large 8vo. Leipzig, Breitkopf & Härtel. Erster Theil, pp. xvi., 341. Zweiter Theil, pp. xii., 562.

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mandato renuntiantur philosophiae doctores et artium liberalium magistri rectore magnifico F. Zarncke decano G. M. Wundt procancellario I. A. H. Leskien inde a die primo mensis novembris a. MDCCCLXXXI usque ad diem ultimum mensis octobris a. MDCCCLXXXII creati. Praemissa est G. Wundtii commentatio patrio sermone conscripta: Das Weber'sche Gesetz und die Methode der Minimaländerungen.]

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(2) *Schlusswort zum ersten Bande*. Philosophische Studien, I., Heft 4, 615-617.

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(5) *Logik*. Eine Untersuchung der Principien der Erkenntniss und der Methoden wissenschaftlicher Forschung. Vol. II. Methodenlehre. Large 8vo. Stuttgart, F. Enke. pp. XIII., 620.

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(2) *Grundriss der Psychologie.* Zweite verbesserte Auflage. 8vo. Leipzig, W. Engelmann. pp. xvi., 392.

(3) *Outlines of Psychology,* translated by C. H. Judd. 8vo. Leipzig, W. Engelmann. pp. xviii., 342.

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(5) *Einige Bemerkungen zu vorstehendem Aufsatze* [von R. von Schubert-Soldern, Erwiderung auf Prof. Wundt's Aufsatz "Ueber naiven und kritischen Realismus"]. Philosophische Studien, xiii., Heft 2, 318-322.

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(2) *Zur Kritik tachistoskopischer Versuche.* [Erster Artikel.] Philosophische Studien, xv., Heft 3, 287-317.

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(4) *Compendio di psicologia.* Traduzione sulla terza edizione tedesca di L. Agliardi. 8vo. Torino, C. Clausen (V. Bona). pp. xvi., 282.

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1902

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(2) *Naturwissenschaft und Psychologie.* Sonderausgabe der Schlussbetrachtungen zur fünften Auflage der physiologischen Psychologie. Large 8vo. Leipzig, W. Engelmann. pp. 124.

(3) *Schlusswort des Herausgebers.* Philosophische Studien, XVIII., Heft 4, 31. März, 793-795.

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1904

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1906

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1907

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(5) *Schallnachahmungen und Lautmetaphern in der Sprache*. München, Bayerische Druckerei und Verlagsanstalt. Beilage zur Allgemeinen Zeitung, Jahrgang 1907, no. 40, 16. Febr., 313-316.

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(12) *Logik*. Eine Untersuchung der Prinzipien der Erkenntnis und der Methoden wissenschaftlicher Forschung. Vol. II. Logik der exakten Wissenschaften. Dritte umgearbeitete Auflage. Lex. 8vo. Stuttgart, F. Enke. pp. xv., 653.

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(2) *Kritische Nachlese zur Ausfragemethode*. Archiv für die gesamte Psychologie, XI., Heft 3-4, 19. Mai, 445-459.

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MINOR COMMUNICATIONS

ON THE ASSOCIATIVE POWER OF ODORS

By J. W. Harris, Ph. D.
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That odors have the power to recall the past with unusual vividness is an observation not infrequently to be met in general literature, and casual experience now and then seems to confirm it. Careful laboratory experiments on the other hand have given no evidence for such a pre-eminence.[1] The mediating fact seems to be that both the observations and the experiments are correct for the special conditions under which they have been made, and both misleading if incautiously generalized. Casual observation here, as usually, is struck by the exceptional and neglects the commonplace; it notes the cases where vivid memories are roused by odors, but fails to observe the vastly more numerous instances in which they suggest nothing old and nothing vivid. The odor of coffee, of kerosene, of illuminating gas, of coal tar, of fresh paint, of tobacco, are to most people no more definitely suggestive of the remote past than the sight of the same articles would be. It is clear, then, that if odors on some occasions give rise to vivid recollections, they must do so in virtue of some peculiarity in the odor or in the circumstances under which it is now or has previously been perceived.

In the writer's belief the essential features (after a suitable apperceptive mood at the time of forming the association and again at its recall) are simply that the odor shall be one that is not too frequently experienced, at least not so frequently experienced under varied conditions, that its preponderant association with one set of circumstances is weakened and blurred out by many others, and, as contributing to this last, an attitude of interest in the odor *per se* and not as merely means to an end.

It is not difficult to find analogous cases. Sense experiences in any field are apt, if they recall anything at all, to call up their circumstances vividly when these are unique and infrequent; but they lose that power when often repeated in many settings. Old tunes recall the past clearly if not heard too often; those heard frequently recall, as a rule, recent circumstances only or none at all. Faces known only in pictures and thus always seen from the same aspect (the common portraits of Washington, for example), are often recalled more definitely than the faces of one's own family which are seen from day to day in a hundred different aspects. In a word, when odors call up the past with definiteness, they probably do so because they are associated with comparatively little else than the circumstances which they recall, and if odors have such limited associations more frequently than sights and sounds, it is because they are less often and

[1]Heywood, Alice, and Vortriede, Helen A.: Some Experiments on the Associative Power of Smells, this *Journal*, XVI, 1905, 537-541; and Bolger and Titchener, this *Journal*, XVIII, 1907, 326-327.

less attentively experienced. They are like ordinary photographs showing one image clearly, while sights and sounds (and "ordinary" odors) are like composite pictures in which so many faces are united that no one is clear above the rest.

The laboratory studies have not taken this feature into view and have therefore not succeeded in reproducing the essential conditions of the phenomenon as it appears casually, though at the same time they have made clear that odors as a class are probably without exceptional reproductive power—except such, perhaps, as may attach to them through their high affective coloration.

The experiments to be described in this paper are, it must be confessed, inadequate to a direct proof of the above hypothesis though they offer considerable indirect evidence for it. The method employed was the very simple one of presenting to the observers a series of odors under conditions which required the formation of certain definite new associations and noting the changes that took place in both the old and new associations as the latter were gradually formed; and later following this up by occasional testings at intervals of from a few days to several weeks and in some cases a number of months. The odors used were twenty-five easily obtainable ones which promised, nevertheless, to be unfamiliar enough not to be instantly named by the observers. No effort was made to select odors representative of the different odor groups of Zwaardemaker, but some of them were agreeable and some disagreeable. The new associations to be formed were with two-place numbers, one for each odor. The odors were placed in small bottles, all alike and all wrapped to the top in exactly the same way with blue paper and all labeled with gummed labels carrying the different numbers in large type.

In making a test the experimenter handed one of the bottles to the observer who uncorked it, took a whiff of the odor and gave its number if he was able (or, in the case of the first presentation, some indication of such associations as were called up by it, if any had appeared). The observer then looked at the number and tried in such manner as he pleased to connect it with the odor, after which he was given another bottle, and so on to the end of the series, when he reported anything with reference to the associations or their formation which he judged worthy of record.

In the earlier experiments the association did not become permanent until after several sittings, because too little attention was given to the number (seen and pronounced but a single time) even when only five bottles were used at a time; the odor, when it came, drove the number completely out of mind. Later when this defect was remedied by concentration of attention upon the number for fifteen or twenty seconds before taking a whiff of the odor a set of five odors could be connected with their numbers at a single sitting, though of course much more practice was required before the number came "direct," that is, came first, and without mediation, to consciousness.

Of the seven observers who served in these experiments two were university men, one a university professor, one a woman physician, one a nurse, one an artist and one a housekeeper. For convenience they will be designated by letter: *A*, *B*, and *F* were men; *C*, *D*, *E*, and *G* were women. Not many tests were made with *G*.

The following introspections reported at the close of the tests from time to time, give some indication as to the behavior of the old or earliest associations. The differences are probably due, as will be shown more fully later, to the mental attitude of the observer toward the whole experiment. The odors at the first sitting called up in *A*'s mind scenes of his childhood and an early apprenticeship in a drug

store; later he remarked that the associations were mainly with previous sittings in the present experiment and that as the odors came "direct" the old associations dropped out and did not even come secondarily; on still another day he spoke of them as being so "far in the background as to have given place entirely to the recent associations." Of somewhat similar tenor were the reports of *C*, the woman physician, who has a very keen sense of smell coupled with a lively visual imagination and has had, of course, daily experience with odors and some reason for attending to them. With this observer the odors at first had both the elements of remote and of recent experience. She used at different times in general introspection the following expressions: "The concrete associations are dropping out but with longer thinking they come back;" "the main associations are with the last time of smelling," the concrete associations are in the fringe of consciousness." At the last time she reported that the old associations were in the periphery of consciousness but staid there through the habit of ignoring them. *C* at first referred nearly all the odors to definite classes in *materia medica*, and the associations with experiences of childhood, though present, were relatively few. She easily became indifferent to the affective coloration of odors that were at first pleasant or the contrary, thus carrying over her professional attitude into a new field. These observations show the attitude that may be acquired toward the associations called up by odors, and at the same time confirm in their measure the hypothesis above propounded that the powerful associative quality of smells is due jointly to the infrequency of their occurrence and to the attitude or purpose with which they are received.

If for any reason the frequent experience of odors leads to the repetition of the old associations rather than the formation of new ones, a strengthening rather than a weakening of the old will naturally result; and something of this kind seems to have taken place with observers *B* and *D*. *B* had great difficulty at first in getting certain of the odors to come "direct" and remarked that the "associations already called up exclude all new ones." After the first three months' interval of no practice *B* said that for him the more ready recall of the number depended rather on the definiteness of the odor [*i. e.* its unlikeliness of confusion with others] than on the recency, yet as the experiment continued the associations were rather with previous sittings than with still earlier experiences. The first associations, had not really dropped out, but came up after the more recent ones.

D's case was even more marked, for she reported an actual reinforcement of the first associations even to the end of the tests. She speaks nevertheless of a little association with other times of testing; and in connection with one odor, smelled again after a long interval, there was a distinct association with a door-bell interruption that had occurred with that odor at an earlier test. The attitude of observer *D* was quite different from that of *C*; her trend of interest was not at all professional but toward the odor itself and what it recalled. The various odors after practice brought their numbers directly indeed, but generally some association with early life or everyday scenes came up also. She did not, like the other observers, have in the tests after the longer intervals, associations with the previous times of learning. The emotional coloring seems to have led, as we have already suggested, to such a repetition and reinforcement of the primary associations as caused their persistence though secondary in point of time. A longer continuance of the practice with the new associations and a practical purpose in forming them would very likely have made them preponderant over the old, even in her case.

F after a five months' interval, on being tested, reported: "Associations in general are very meagre. However, all but one [of twenty odors] seem familiar. . . . . Visual imagery, except in two cases, is not present at all. . . . . There is really much less imagery than when I learned them and [in response to a question] there was not a single case of association now with the times of learning." The introspections of the other observers furnished little evidence on these points.

With reference to the formation of the new associations the experience of all the observers was tolerably uniform and such as one might expect it to be. The differences between the different subjects were mainly quantitative. In most cases the old associations tended in course of time to drop out and leave the name and the number to struggle together. The association of the odor with the number generally went through three distinguishable stages. In the first the name given to the odor, in many cases accompanied by visual imagery, would rise at once on the perception of the odor and be followed immediately by the number. The announcement of the observer in such cases would be, *e. g.*, "Tar—84." Such a stage would continue from 5 or 6 to 15 or 20 days, varying with the observer, the affective quality of the odor, the pitch of attention, the degree of familiarity, the general attitude of the observer, etc.

The transition from this first stage to the second was a gradual weakening or recession of the name, and a gain in certainty and promptness in the number.[1] In many instances, though not in the case of all associations that attained "directness," there came after the first a clearly marked, but often short, second stage when the word and the number seemed to rise together. This "simultaneous" phase did not often last more than one or two days, but there was more or less tendency to relapse into it when "directness" had been attained but not fully established. In the first stage there was often some discouragement, and such statements as "I believe these will never come 'direct'" were not unusual. When the "simultaneous" stage came there was a feeling of encouragement or success which seemed to hasten progress. In the third and last stage the association with the number was prompt and without mediation. The observer's announcement was: "84—tar." With practice the name retired still further into the background, coming later and later, until it finally ceased to appear, and the odor called up the number only.[2]

After intervals of rest of a week or more, and in the earlier stages even after the interval from Saturday to Monday, the new associations seemed noticeably weakened. *A*, for example, after an interval of ten days, when most of the numbers were nearly "direct", thought that all associations with the numbers had become a little uncertain as compared with what they were before. In at least two other instances, however, number associations once established showed great "directness" and persistence. *B* reported that several months after his last test he happened one day to get an odor to which he at once said only "76," and no old name followed. At about the same time *E*, fully two months after her last test, found spearmint growing wild. She caught the odor and at once said "63." In these we have apparently

[1] *A* remarked at an early stage that if for any reason the name was hindered in rising the number stood ready.

[2] Complete unfamiliarity in the odor favored the prompt association of it with its number. To *E* number 41 was so unfamiliar that she had no other name or association with it and hence the number 41 served itself as a name and became almost immediately "direct."

chance cases on a small scale of the same "unusual" reproductive power of odors which we are endeavoring to explain.[1]

With certain subjects a prominent factor in the learning of the numbers was the formation of mnemonics, for example, "63, three, tree, herb, mint." While of some advantage in the first fixing of the numbers, these often seemed to hinder in the "coming direct," the mnemonic links being especially persistent. Others made use of incidental helps. *C*, for example, was inclined to set the odor in its place in the series as given at the first test. *A* had also a similar tendency, and when two groups of five were thrown together he confused the two that had been originally at the middle of the separate groups. He also noted other general confusions from running two groups together. *B* also, after he had had two or three different series of five, would fumble considerably at the beginning of any test if he had not been told that the first, second or third series was to be used. Two or three mistakes might be made (imaginations of other series) until the proper set of associations was brought into play by some odor more distinct than the rest. *F* presented the unusual but interesting ability to give the approximate numerical location of any odor in the entire series, at the same time showing considerable difficulty in getting the numbers exactly, saying, for example, "Well, that's in the fifties," or "that's up near the nineties." He could not give any definite introspection as to his manner of locating them, but it seems to point to some sort of number-form, influential though far in the periphery of consciousness. *A* also, who has a definite number-form, had something of the same power of approximate location.

The affective qualities of the odors seem to have played a certain subordinate role in the fixing of the associations, some distinctly pleasant (and in a few instances some distinctly unpleasant) having formed especially prompt and lasting associations with their numbers; but our evidence on this matter is meagre. Certain remarks of the observers suggest, also, another and closer point of connection of affective qualities with our main question. Odors, as a rule, carry a strong affective coloration, but this is weakened and in some cases nearly disappears with frequent experience of them. With *C*, it was noted as has been said that the feeling element seemed to fall away as testing with the odors was continued. It also returned again in a measure after about a month's interval of no work with them, as was observed on two separate occasions. It would seem, therefore, that the feeling coloration of the odors and their tendency to recall early associations run a somewhat parallel course, which next suggests the question whether the feeling is not itself an important link in the recall. The data furnished by the present experiment throw no further light upon the matter, but an affirmative answer would seem not unlikely.

[1] Observer *C* showed the interesting case of two odors the numbers for which had not yet become established as "direct," but nevertheless rose without intermediary after a six weeks' interval.

MYSELF AND I: A CONFESSION

By L. G. WINSTON

Somewhat after the order of Dr. Jekyl and Mr. Hyde I seem to possess two distinct personalities, being both at the same time but presenting no such striking contrast as the Jekyl-Hyde combination. They are about equally virtuous. Their main difference seems to be one of age, one being a decade or so in advance of the other.

At times they work harmoniously together and again at cross purposes. I do not seem to have developed equally. Part of me sits humbly at the feet of the other part of me and receives advice and instruction. Part of me feels constrained to confess to the other part of me when it has done wrong and meekly receives rebuke. Part of me tries to shock the other part of me and to force the more dignified part to misbehave and giggle and do things not considered correct in polite society.

My younger part delights to tease the older, to doubt her motives, to interrupt her meditations. It wants to play while my older self is more seriously inclined. My younger self is only twelve years old. This is my real self. To my own mind I am still a little girl with short dresses and a bunch of curls. For some reason my idea of self has never advanced beyond this point. The long dress and the hair piled high will never seem natural. Sometimes I enjoy this duality and again I do not. Sometimes the two parts mingle delightfully together, again they wrangle atrociously, while I (there seems to be a third part of me) sit off and watch the outcome.

The older part gets tired before the younger. The younger still fresh and in a good humor undertakes to furnish amusement for the older. I have often thrown myself on the bed wearied and exhausted and been made to shake with laughter at the capers of the younger part of me. They are capers indeed. On these occasions she will carry on conversations with friends—real friends—fairly bristling with witticisms, and although taking both parts herself, the parry and thrust is delightful.

Sometimes, however, the younger part of me seems to get up all awry. She will carry on quarrels—heated quarrels—from morning till night, taking both sides herself, with persons whom I (the combination) dearly love, and against whom I have no grievance whatever. These are a great distress to my older self.

On other days she seems to take the greatest delight in torturing me with imaginary horrors. She cuts my throat, pulls my eyes out of their sockets, removes tumors and amputates limbs until I wonder that there is anything left of me. She does it all without administering anæsthetics and seems to enjoy my horror and disgust.

Again some little jingle or tune will take her fancy—and she will repeat it to herself until I am almost driven to madness. Sometimes it is only a word, but it seems to have a fascination for her and she rolls it as a sweet morsel under her tongue until sleep puts an end to it.

Again, if I (the combination) fall ill, one part of me, I have never discovered which, invariably hints that I am not ill at all but merely

pretending. So much so that it has become with me a recognized symptom of incipient illness.

Moreover, the younger and older are never on the same side of any question. One leans to wisdom, the other to fun. I am a house divided against itself. The younger longs to dance, to go to the theatre and to play cards, all of which the older disapproves. The younger mocks the older, calls her hypocrite and the like until the older well-nigh believes it herself and almost yields to her pleadings. The older listens sedately to the sermon, while the younger plans her Easter suit or makes fun of the preacher.

The older declares she will never marry, while the younger scouts the idea of being an old maid. But even if she could gain the consent of the older it were but little better, they differ so as to their ideals.

In society the difference is more marked. I seem to be a combination chaperone and protege. The older appears at ease, the younger shy and awkward, she has never made her debut. If one addresses a remark to her she is thrown into utter confusion until the older rushes to the rescue. My sympathy is with the younger, however, for even to this day I, the combination, can scarce resist the temptation to say nothing when there is nothing to say.

There is something tragic to me in this Siamese twins arrangement of two so uncongenial. I am at one and the same time, pupil and teacher, offender and judge, performer and critic, chaperone and protege, a prim, precise, old maid and a rollicking school girl, a Tomboy and prude, a saint and sinner. What can result from such a combination? That we get on tolerably is a wonder. Some days, however, we get on admirably together, part of me paying compliments to the other part of me—whole days being given to this—until each of us has such a good opinion of herself and the other that we feel on equal terms and are at our happiest.

But how dreadful are the days when we turn against each other! There are not words enough to express the contempt which we feel for ourselves. We seem to set each other in the corner and the combination as a whole is utterly miserable.

I can but wonder and enjoy and wait to see what Myself and I will make of Me.

PSYCHOLOGICAL LITERATURE

Grundlinien der Psychologie, von DR. STEPHAN WITASEK. Mit 15 Figuren im Text. (Philosophische Bibliothek, Band 115.) Leipzig, Verlag der Dürr'schen Buchhandlung, 1908. pp. viii, 392. Mk. 3.00.

This little volume has a twofold claim to consideration: first, as a text-book for class instruction, written by one of the best known and most competent of the younger generation of psychologists, and secondly as a summary of the views held by that Austrian school of which Höfler and Meinong are the leaders. To the present reviewer, the latter claim appears definitely to outweigh the former. We do not doubt that the book could be used with success, in undergraduate work, by its author or by a teacher of like training; and we have nothing but admiration for many of its features—its resolute facing of difficulties, its solid packing of fact, its caution and suspension of judgment on various disputed points. But we find, on the other hand, a marked unevenness of style, ranging from the colloquial simplicity of the primer to argumentation on the highest technical level; we find that the ideal of complete exposition, praiseworthy in itself, has too often led to the dryness and heaviness of an encyclopædia article; and we find ourselves compelled, as the book proceeds, to master a highly articulated terminology, which is not applied in such detail or with such frequency as to become wholly familiar, and which must therefore tend to substitute, in the beginner's mind, a psychology of reflection for the psychology of observation. In these respects the book is distinctly inferior to Ebbinghaus's *Abriss*. It is mainly interesting, then, as a compendious statement of the current psychology of the Austrian school. Höfler's *Psychologie* and *Grundlehren der Psychologie* appeared as long ago as 1897; and although the general attitude of the school has remained unchanged, the progress of psychology during the last eleven years has naturally served to extend, modify, revise and clarify its fundamental ideas. In this work of systematization, the author of the *Grundlinien* has himself played an active part.

The book has two main divisions: a shorter, entitled General Psychology, which occupies 96 pp., and a longer, Special Psychology, which occupies 270 pp. Ch. i of the former division defines the province of the science. The subject-matter of psychology is the sum-total of mental facts; and the characteristic feature of anything 'mental' is that it points beyond itself to a 'physical.' "Unser Vorstellen ist so beschaffen, dass es uns *Dinge* zur Vorstellung bringt:" "mein Denken erfasst Dinge, die selbst kein Denken, ja überhaupt nichts Geistiges sind. . . . Das Gleiche gilt vom Fühlen und vom Wollen." This 'pointing to' is the most obvious indication of that essential difference between the two realms of fact of which one can, in the last resort, say nothing more than: "Materielles hier und dort Geistiges." Its explanation is relegable to epistemology. So much on the positive side. On the negative, the author rejects both the older definitions of psychology as the 'science of mind' and the more recent definition which makes it the 'science of the subjective aspects (properties, attributes) of experience' (Mach, Ebbinghaus, Külpe).

Ch. ii discusses the theories of the relation between mind and body.

The author writes carefully and with reserve, though his leaning towards interactionism is sufficiently clear; psychophysical parallelism, we are told, must, if consistently carried out, lead to the acceptance of a 'substantielle Seele' (p. 40). But it might, on the writer's own principles, just as well lead to a non-substantial 'unconscious;' and its representatives deny emphatically that it leads them either to the one or to the other. The chapter as a whole, while it achieves a difficult task of condensed writing, is far above the level of a brief text-book and far too scrappy for a first-hand discussion.

Ch. iii deals with the concepts of mind, self or ego, and unconscious. From the psychological standpoint, there is no ground for the assumption of a 'substantial mind,' unless we adopt the principle of parallelism; in that case, we must have recourse to a mind-substance, which, however, is not simple but compound (*zusammengesetzt*). The further question of an 'ultimate' simple mind belongs to metaphysics. The psychological self or ego comprises the sum-total of the individual's 'actual' mental facts, and the sum-total of his *Dispositionsgrundlagen*, of the substrata of his mental dispositions. Mental dispositions are not 'unconscious' mental facts; they are, as such, simply faculties or capacities; the only 'real' things that belong to them are constituents of the organism that co-operate, as relatively permanent part-causes, in the realization of the mental activities of the individual. These 'constituents of the organism,' *Dispositionsgrundlagen*, are most naturally regarded by interactionism as physical (cerebral), while parallelism, as has been remarked, must look upon them as mental (manifestations of the mind-substance). 'Unconscious' mental facts are actual, but of such low intensity that they cannot be noticed (*bemerkt*); there is no need for any other interpretation of the term.

Ch. iv undertakes a preliminary classification of the subject matter of psychology. We have to distinguish: first, between the fundamental mental formations (*Gebilde*) and the mental processes (*Prozesse*). The formations fall into two great groups: the intellectual and the emotional. In the former class we find ideas and thoughts; in the latter, feelings and appetitions. But further: we must distinguish, in every fundamental formation, "zum mindesten in gewissem Sinne," between act and content. Thus there is a certain 'part' of the idea by virtue of which it brings a definite object to consciousness; this is its content. There is also a part in virtue of which we recognize it as idea, in contradistinction (say) to feeling, or in virtue of which we mark off idea of perception from idea of memory or of imagination; this is its act. Content and act are, in the idea, inseparable, and both alike are mental; they are thus both different from the 'object' of the idea, which is usually physical.

So far the author. And here the reviewer must interpose his objection to the separation of 'act' and 'content,' as at once unnecessary and confusing. Other psychological systems, as is well known, get on without any such dichotomy of the mental life; and it does not appear that Dr. Witasek gains by its introduction. He writes, as always, carefully; doubtless with especial care, in view of criticism past and to come. But his exposition, nevertheless, comes perilously near to self-contradiction. Every fundamental formation is ideally separable into act and content, "at least in a certain sense." In what sense? Turn to the special psychology of feeling, p. 318. Here feeling proper, *das Gefühlsmoment* in the affective complex, is all act; quality and intensity of feeling are quality and intensity of act, not of content; the content of feeling is not affective content at all, but ideational content. Or turn to the special psychology of thought, p. 280. Judgment divides ideally into act of judgment and content of

judgment. The act has two essential moments, conviction and affirmation-negation. The content of the judgment is not judgment-content, any more than the content of feeling is feeling-content; it is, again, ideational content. But there is a further complication. The 'contact' of ideational content with the affirmation-negation moment of the act brings into being a new quasi-content, the objective. This objective is not subject-matter for psychology; it is, however, psychologically useful as indicating the way in which the act of judgment approaches and connects with the (ideational) content of judgment; thus, we distinguish by its means between the thetic and the synthetic function of the act of judgment, the behavior of the act in the existential and categorical judgments of the logic text-books. For the rest, the conviction-moment of the act is capable only of intensive gradation; the moment of affirmation-negation is qualitative in character. Between the qualitative extremes lie qualitative transitional forms, the middlemost of which is a suspense of judgment (p. 285); it is, however, not clear to the reviewer whether these intermediate qualities belong purely to the moment of affirmation-negation or represent combinations of this with the moment of conviction (pp. 79, 283 f.). Further: certain judgments (not all) evince in the act of judgment a third moment or attribute, that of evidence. This may appear as evidence of (objective) certainty, correlated with affirmation or negation; or as evidence of (objective) probability, correlated with the qualitative transition forms of that moment; whether evidence itself is quantitatively or qualitatively graded is not expressly said, but the gradation would seem to be qualitative.

The underlying idea of all this classification may be simply expressed in the sentence: 'mental facts' point beyond themselves (1) in various ways (2) to different things. The ways come to consciousness as act, the things as content. But a first point of criticism, which at once suggests itself, is that the typical function of pointing-towards is exercised only by the idea; the idea is assumed to be the typical mental formation. At the beginning of the book, mental fact is defined by reference to idea, and the other varieties of mental formation are listed, so to say, as an appendix (pp. 6 f.). When the distinction of act and content is first drawn (pp. 73 ff.), we are left 'doubtful' whether the content of feeling, wish, etc., is directly or indirectly given; given, that is, as object-brought-to-consciousness by the pointing of feeling-act to object of feeling, or given secondarily, as object-already-brought-to-consciousness-by-act-of-idea. When we come to the special psychology of feeling and judgment, the doubt has disappeared; the content is ready-made ideational content, and the acts of feeling and of thought do not aim directly at objects at all. In other words, the original definition of 'mental fact' has, in the writer's own exposition and quite apart from any further question of its validity, been modified to meet the cases of feeling and judgment; or, to invert this statement, the treatment of feeling and judgment has been forced into the terms of a definition which is inadequate to them. A second point of criticism is suggested by the definition of act. The act of idea or judgment (p. 75) is that part or aspect of the formation which is directed towards the object; it is a 'relation' or a 'reference' in the idea of judgment. Now references may be qualitatively dissimilar; but can they differ intensively? Can there be more and less of a particular manner-of-pointing? If the reply is made that such a state of affairs, while logically impossible, is possible in psychology, then we are brought face to face with the real issue involved in the whole classification, the ultimate question of psychological fact. Does introspection compel us to separate act from content, whether in the idea or elsewhere?

And is Dr. Witasek photographing consciousness or imaginatively constructing it? Does he offer us *Beschreibung* or *Kundgabe*, *Erlebnis* or *Ausdruck?* In the reviewer's opinion, his act-and-content psychology is the artificial product of a wrong initial attitude: the attitude of one who is not content to regard mind as a datum for scientific description and explanation, but who reads his epistemology at every point into his psychology.

To return, however, to Ch. iv. The mental 'processes' come to consciousness as sequences, continuous or discrete, of fundamental mental formations. They may be classified, empirically, by reference to the act (of idea, of thought, of feeling, of appetition) with which they begin and end. Thus, if the process runs from idea to idea we have 'association;' with other first and last terms we have rumination, comparison, integration, attention, inference, vacillation, deliberation, resolution, etc., etc. It is, however, characteristic of all such processes that they are not adequately defined by reference to consciousness; their essentials lie outside consciousness, in the brain (interactionism) or in the unconscious depths of the substantial mind (parallelism). Further: since the mental formations themselves depend in part upon extra-conscious conditions, and since these conditions are most plausibly identified with the extra-conscious essentials of the mental processes, we may consider idea, thought, feeling and appetition, from the point of view of process, as ideation, thinking, being-affected, desiring. This reading of formation as process gives us a definite point of attack for our process-psychology; beginning with the formation process, we may gradually advance to a theory of the processes *sensu stricto*. In the meantime, we note that processes obviously divide into active and passive, and that the active tend (in course of practice) to lapse into passive; so that the same mental result may represent very different expenditures of mental force; or very different amounts of mental work.

Besides formations and processes, psychology has to take account of mental dispositions. These, as we have noted, are not themselves in any sense real things. They are *Kausalrelationsfälle;* they express the fact that the individual contains within him a part-cause of his mental processes and formations. The two principle problems of this division of psychology are, first, that of the number of fundamental dispositions (whether, *e. g.*, memory and imagination are, as dispositions, identical or different), and secondly that of dispositional change (practice, fatigue, etc.; change by acquisition of new dispositions).

Ch. v, the concluding chapter of the division of General Psychology, deals with the problem and method of psychology. As we find here nothing new or characteristic, we may pass directly to a few concluding remarks upon the division of Special Psychology. Here we have four chapters, named from the four fundamental formations, idea, thought, feeling and appetition; there are no special chapters devoted to the processes and dispositions. Of the 270 pp. which are assigned to the division, 182 fall to the lot of idea (sensation, 'produced' idea or *Gestaltqualität*, memory and association), 36 to thought, 34 to feeling, and 18 to appetition. The dominance of idea is natural, since experimental psychology has gathered a vast store of facts concerning sensation and memory; we have seen that it is also, from the author's standpoint, justifiable,—idea being the typical 'mental fact.' Thought, on the other hand, is somewhat shabbily treated. And when we note that attention receives hardly nine pages, while the *Annahme* gets five and a half, we are tempted to accept the challenge of the Preface, and to judge the book not only by what it contains, but also by what it omits. The final chapters, on feeling and appetition, are written with

the critical caution to which we have grown accustomed. Nevertheless, more might surely have been made of the appetition; and the average reader would probably be glad to exchange many pages of tentative articulation for a frank and clear statement of opinion.

This whole division makes, in fact, a mixed impression. The careful advance of the close-packed paragraphs is, in its way, admirable. Yet our intellectual palate is not satisfied; we want something more,—that solid and conclusive something, perhaps, which Dr. Witasek denies us, on grounds of general scientific validity, in the brief *Schlusswort*. But it may be that we should have been content with a couple of special chapters on the processes and the dispositions. The discussion of these topics is scattered here and there throughout the four chapters, and not even the unusually good index makes any attempt to round them up; under *psychische Prozesse*, *e. g.*, there is no reference either to *Aufmerksamkeit* or to *Produktion der Vorstellungen*, under *Disposition* there is no reference to *Reproduktionsdisposition*. The result is that we have no unitary attempt at the solution of problems which the author emphasized as of high importance in ch. iv.

P. E. WINTER.

Vorlesungen zur Einfürung in die experimentelle Pädagogik und ihre psychologische Grundlagen, von ERNST MEUMANN, 2 vols., Leipzig, Engelmann, 1907. pp. 555, 467.

The psycho-pedagogical movement, which dates back about two decades, and which is known in America as "child-study," has been crystallizing in Germany, within the last few years, into the systematic form characteristic of German scholarship, under the double name of Child-psychology, and Experimental Pedagogy. Experimental Pedagogy is child study on a higher plane of scientific development, or perhaps one should say, narrowed down into a more or less exact science, showing its distinctive feature in the application of the methods of experimental psychology to the problems of education and instruction. The investigations in the field had been casual and fragmentary, until the "Experimental Didactics" of Lay appeared in 1903, the first attempt to bring the results of various experimental studies together into a coherent form, and to build upon them a system of scientific pedagogy. Now we have a second work in a similar line, though of somewhat different character, in the "Introduction to Experimental Pedagogy and its Psychological Foundations," by Prof. Meumann.

The book consists of a series of lectures given originally before the Teachers' Union at Kœnigsberg, revised and supplemented for the present publication. The author does not pretend to present a completed system of pedagogy nor even a comprehensive and exhaustive survey of all that has been done in the field of Experimental Pedagogy, but intends, rather, to show, in a popular and brief form, the methods employed in the experimental investigation of pedagogical and didactic problems with illustrations of some of the results already attained. In the course of his lectures, however, he steps out, quite naturally, from these limitations and often seems inclined to speak the final word upon practical questions.

His intellectualistic and formalistic standpoint sound the keynote of the whole book, as the voluntaristic and motoristic philosophy gave that of Lay's "Experimental Didactics." Meumann's work is, however, far less overtly philosophical and his own pedagogic views are much less explicitly asserted than those of Lay.

In the first chapter are discussed the character, problems, peculiar position, and methods of experimental pedagogy, which is defined as experimental investigation in the field of education, and aims at the

final, or, rather, progressive, establishment of a "scientific pedagogy" by means of "systematic observation and experiments." The lengthy defence of systematic observation and experimental methods here made seems to us superfluous; but was very likely appropriate to the audience to which these lectures were originally delivered, and is, at any rate, not unusual in German scholarship. The second chapter is devoted to a general view of the topics which the writer is to treat in the rest of his book. The third chapter treats the physical and mental development of the child, in its general course, characteristics and periodical fluctuations, with the pedagogical bearings of these. In the next four chapters the author goes into the discussion of the development of particular mental faculties, making a comparison between those of the child and the adult. Attention, sense-perception, the perception of space and time, the ideational content of children's minds at their entrance into the school, the development of memory power, representation processes, the development of speech, the growth of the emotional and volitional life—all these receive more or less full treatment. The eighth, ninth and tenth chapters are given to the question of individual differences and types, and close the first volume of the work.

The eleventh chapter, beginning Vol. II, is concerned with scientific methods of mental work and acquisition, its economy, technique and hygiene. The next chapter, as a continuation of the preceding one, is devoted entirely to the study of fatigue. With the thirteenth chapter the author enters upon the problems of special didactics, devoting one chapter to each of the following branches of elementary instruction: Object lessons, reading, writing, number work and drawing. Here he merely shows what can be done in these lines by means of experimental investigation. In the eighteenth, the concluding chapter, he turns to the future of experimental didactics, showing the possibility of the extension of experimental investigation to the higher branches of school study.

The book is written in a clear style, and furnishes easy and agreeable reading, although too minute analysis and not infrequent repetitions are something of a blemish. Its strength lies in the fact that Meumann, as one of the most eminent and vigorous investigators in the field, supplements the results of previous studies with those of researches made by himself or under his immediate direction. Psychologists, as well as experimental pedagogists, might have wished a more detailed account of some of these individual studies, the results of which he publishes here for the first time. Beginners in experimental pedagogy, for whom the book is intended, will, however, find in it an excellent guide to the nature, and the present attainment of this new branch of pedagogy. In fact this is the only book of its kind as yet published. The student of psychology will find those chapters of most interest, in which Meumann treats of memory, attention, association and individual types. T. MISAWA

The Psychology of Feeling and Attention, by EDWARD BRADFORD TITCHENER. The Macmillan Co., New York, 1908. 404 p.

This book consists of eight lectures delivered at Columbia in February, 1908, together with many appended notes. The topics are: 1. Sensation and its attributes; 2. Sensation and affection, the criteria of affection; 3. The affections as *Gefühlsempfindungen;* 4. The tri-dimensional theory of feeling; 5. Attention as sensory clearness; 6 and 7. The laws of attention; 8. Affection and attention. There are some seventy-five pages of notes on various lectures. It is entirely impossible to do justice to a work of this kind in a brief review. It bristles with technicalities and abounds with quotations in French

and German; and more or less of the discussion pertains to matters of method. The most constructive part of the book (pp. 291-3) is the statement of the theory that the material of consciousness or the stuff out of which mind is made is ultimately homogeneons. The affections are of the same general sort as sensations, only they are not developed into them. The affections might thus be called undeveloped sensations but for a verbal difficulty. The peripheral organs of feeling are the free afferent nerve-endings distributed among the inner organs of the body, and these endings represent a lower level of development than the specialized receptive organs. "Had mental development been carried further, pleasantness and unpleasantness might have become sensations; in all likelihood would have been differentiated, each of them, into a large number of sensations. Had our physical development been carried further we might have had a corresponding increase in the number of internal sense-organs." This explains the obscurity of feeling. Affective processes are those whose development has been arrested. The feelings never report the tone of the bodily system from which they proceed and can only vary between the terms good and bad. These reports vary in degree but cannot in kind. And, finally, this theory explains the introspective resemblance between affections and organic sensations. (This note will not preclude longer or more adequate review later.)

Notes on the Development of a Child; II. The Development of the Senses in the first Three Years of Childhood, by MILICENT WASHBURN SHINN. University Press, Berkeley, July, 1907. 258 p. (Univ. of Cal. Pub.)

This long delayed publication is most welcome to those interested in this department of work. It shows that the author's observations and her inferences therefrom and also her reading upon these subjects have been no less careful than upon the topics upon which she has previously published. As an observer Miss Shinn is past master. One cannot, however, but wish that her reading and thought in the line of comparing what others have written upon the subject were a little more developed. As it is, her work is a contribution of really more original acumen, diligence, and scientific value than Preyer's, but after all the work that has been done there ought to be certain, at least tentative, conclusions drawn and at least provisional summaries of results up to date, with perhaps definite statements of problems next in order. The absence of this we consider the chief weakness in Miss Shinn's paper.

Social Psychology—An Outline and Source Book, by EDWARD ALSWORTH ROSS. Macmillan Co., New York, 1908. 366 p.

In this book, extensive rather than intensive, the author first treats of the nature of social psychology; then suggestibility, its relations to sex, politics, public opinion; then crowds (the individual wilts and thought is arrested), the Kentucky revival, non-morality of crowds; comparison of city and country. In the chapter on the mob mind, crazes and fads, children's crusade, Milan's women's crusade, Mrs. Nation, stampede, financial crazes, are discussed. Prophylactics must make us crank-proof. Sane teachers and the classics, avoidance of sensational newspapers, country life, familism, ownership, pride, love, avoidance of yellow religion, are sanative. Then follow chapters on fashions, nature of conventionality in which effects of caste are discussed, snobbery, stigma on toil, the spirit of the age, why it is unwomanly for women to use stimulants, etc. In the eighth chapter the laws of conventional imitation are discussed along with the flagellants, dancing mania, jumpers, spread of disease, drunks, sex

inflammability, contagiousness of ideas, Tarde's law, the Americanization of Porto Ricans, late marriages, child emancipation, aristocracies, power-holders, pace-setters, dollarocracy, barbarizing influences of the smart set, high potential of the city, spread of ideas of equality, custom, historic continuity, Western spirit, etiquette or sway of custom in ancestor worship, social isolation, sedentary life, anarchism, schools and movements in philosophy, art, the curative power of free discussion and polemics, results of conflict, union, deadlocks, compromises, feuds, the relations between public opinion and social tradition, why an equilibrium is not reached.

The Mental Recreations of the Mental Nurse, by R. JONES, M. D. The Journal of Mental Science, July, 1908.

Dr. Jones pleads for periods of complete change of thought for mental nurses. There should be pensions and several weeks annually for vacation, with at least weekly outings through the year, because recently nursing has become far more exacting and involves more risks, troubles, and anxieties. The required training is longer, the degree of efficiency higher. There ought to be physical and also mental recreations, *e. g.*, golf and bridge. The nurse must keep up hope, for her buoyancy sustains the patient. Reading aloud is an important art, and there are nurses' reading circles, presses and a syllabus. Musical culture, interest in pictures upon the wall, healthful ideas generally, the cultivation of the kodak, and perhaps, especially, a thorough religious spirit. All these are important.

Manual of Psychiatry, by J. ROGUES DE FURSAC. Translated by A. J. Rosanoff. Second American from second French edition, revised and enlarged. John Wiley & Son, N. Y., 1908. 406 p.

This writer is very friendly to the new classification of mental diseases introduced by Kraepelin. Most cases indeed, when carefully examined, can be assigned to some of his groups, although of course there are difficulties. The first part treats of general psychiatry, with chapters on etiology, symptomatology, and the practice of psychiatry; while the second part deals with special psychiatry—deliria of infectious origin, psychoses of exhaustion, acute alcoholic intoxication, chronic alcoholism, chronic intoxication by the alkaloids, psychosis of auto-intoxication, acute and subacute thyrogenic psychosis, dementia præcox, chronic delusional insanity, general paresis, disorders due to organic cerebral affections, psychosis of involution, manic-depressive insanity of various types, reasoning insanity (Kraepelin's paranoia), and constitutional psychopathies or mental disorders of epilepsy and of hysteria, and arrests of mental development. The work certainly has the advantage of being brief and lucid.

The Borderland of Epilepsy, by WILLIAM R. GOWERS. P. Blakiston's Sons & Co., Philadelphia, 1907. 118 p. (Lectures on Diseases of the Nervous System, 3rd Series.)

The author has for years kept a special list of all his border-land cases that were near epilepsy but not of it, and he here presents a collective and more or less systematic study of these cases. The symptoms are fainting, syncope, vagal and vaso-vagal attacks, tetanoid spasms, sudden vertigo often involving loss of sight, consciousness, and sense of impulsion, attacks during sleep, pseud-aural border-line epilepsy. He gives a whole chapter to migraine—its alternations, premonitory symptoms, isolated, prodromic symptoms, during pain, somnolence, elaborate premonitory symptoms, night terrors, somnambulism, half-waking states and narcolepsy. These matters are discussed with the author's characteristic good sense and judgment.

Der Tod aus Altersschwäche, von HUGO RIBBERT. Friedrich Cohen, Bonn, 1908. 85 p.

Death from old age is a result of anatomic and functional changes in the composition of the body due especially to the high differentiation of the intercellular substances. It is a necessary result of chemico-physical properties that underlie life. Death from old age is a totally distinct thing from that by disease and is often unaffected by it, although scelerosis may accelerate it and other diseases may bring complications. Natural death is the death of the brain; a high degree of atrophy of the ganglion cells cannot be harmonized with life. Normally all men live approximately the same time, and the variations are what are to be accounted for.

Der Selbstmord als Spekulation des modernen Verbrechertums, von FRANZ CHYLE. Huber & Lahme, Vienna, 1908. 31 p.

The only cure the author sees against the growing tendency to suicide is the reform of family life, especially in the field of feeling and sentiment rather than of the understanding. There can be no doubt that in many respects civilized man is now beginning to degenerate, and that civilization is taking on certain morbid traits. It is fatal to expect too much of life, to be born rich or to be overloaded with culture too early. Suicides have often cursed their parents, holding them responsible for their deeds.

Untersuchungen zur Kenntnis der psychomotorischen Bewegungestörungen bei Geisteskranken, von KARL KLEIST. Werner Klinkhardt, Leipzig, 1908. 171 p.

This is a very valuable study based largely on one case, which seemed to pass through three akinetic stages, involving apraxia and mutism. The author thinks that psycho-motor disturbances are due to troubles of innervation, and is bold enough to do justice to this now too often discarded idea. The third part takes up the psychic conditions of psycho-motor disturbances in organic sensations, association, attention, etc. The writer is a disciple of Wernicke's and emphasizes his consciousness of corporeity, the substratum of which is a reflex system, including muscular and vestibular sensations, with their correlative motor reactions. Psychological investigation points to the same brain areas as does the analysis of motor disturbances. The frontal regions of the brain are focal for the body sense. The author holds to the dependence of psycho-motor symptoms upon effort, fatigue and other organic sensations, as well as feelings and attention.

Unverbesserliche Verbrecher und ihre Behandlung, von SHIGEMA OBA. Hermann Bahr, Berlin, 1908. 84 p.

The chief plea of this writer is that penalty should be proportional to the guilt and must be in the same field in which the crime was committed. Crimes involving pain should be punished with pain; simple theft by fines, according to the heinousness of the deed. He would also have what he calls preliminary punishments, and thus those under possible sentence should be held to a higher degree of accountability. Warnings and threats this writer thinks, therefore, are of great importance.

Sexuelle Ethik, von AUGUST FOREL Ernst Reinhardt, Munich, n. d. 56 p.

This is a lecture originally given in Munich in 1906, which has reached a circulation of thirty thousand. In this last edition, the author has appended a number of concrete cases, from his experience, ethical-sexual conflicts, that have come to him. There are, for in-

stance, cases of morbid jealousy, of drunkenness and love, of a man who must either refrain from marrying the girl he loved or give up his place in the army, cases of notomanie, of growing discord and incompatibility in married life, of diseased men who wish to marry, of urnings, impotence, nymphomania, hypochondria, satyriasis, hereditary tendencies to disease (whether such should marry), fetishism, effects of reading very plain literature like this lecture, etc. The author's general law, which he urges at the end, is that the sexual nature should neither directly nor indirectly injure any one either of the present or the coming generation.

Geschichte der Philosophie, von MATTHIAS HAMMA. Theissingschen Buchhandlung, Münster, i. W., 1908. 83 p.

This is the tiniest general history of philosophy known to the writer of this review. It ends with Hegel and his immediate followers, and the first edition was issued in 1876; and the second, thirty-two years later, is not much changed. It begins with Thales and includes Schopenhauer and Herbart. It ought to be translated.

Bestia sum: Einige Kapitel über die Kehrseite des Menschtums, von WILHELM WACHTER. Emil Felber, Berlin, 1908. 261 p.

This rather startling title is perhaps quite justified. The author first describes man in primeval times generally, then follows an elaborate chapter on the murder of children among the lower races, and another on cannibalism. In a very striking final chapter, the author draws contrasts and similarities between primitive and civilized man, that are unique and somewhat startling.

Insomnia and Nerve Strain, by HENRY S. UPSON. G. P. Putnam's Sons, New York and London, 1908. 142 p.

The author first gives illustrative cases; then the effects of sleeplessness and fatigue upon the emotions, reflexes, sub-consciousness, atavistic symptoms, inhibition, convulsive seizure and choreic spasm, vascular potential, nutrition, vitality, epicritic neuro-psychoses, with a concluding chapter on prognosis, therapy, predisposition and heredity.

Contribution à la pathologie des mystiques, anamnèse de quatre cas, by FRANCOIS DA COSTA GUIMARAES. Jules Rousset, Paris, 1908. 51 p.

This is an interesting study of four cases of pathological mysticism, chosen and described from the literature, viz.: Plotinus, Fra Angelico, Susanne Labrosse (d. Paris, 1821, age 64), and Eustelle Harpain (d. 1842, age 28). The author's treatment of these cases is rather summary, and he perhaps rather too modestly declares that he will not attempt to draw any definite conclusions from his material.

Recent Progress and Present Tendencies in Comparative Psychology, by ROBERT M. YERKES. Rep. from The Journal of Abnormal Psychology, February-March, 1908. pp. 11.

Yerkes makes four fields of comparative psychology which he would have studied comparatively: first, physiology of the central nervous system in relation to the behavior of organisms and to consciousness, such as the work of Sherrington and Franz; second, descriptive behavior, illustrated by Jennings and Bohn; third, the analysis of animal psychic processes, shown in works like those of Watson, Porter, Cole, and Berry; and lastly, discussions of the basis and method, such as the work of Claparède, Washburn and Yerkes. The most recent works of each of these authors are briefly described.

A Non-Surgical Treatise on Diseases of the Prostate Gland and Adnexa, by G. W. OVERALL, M. D. Rowe Publishing Co., 1906. pp. xii, 228.

This is the third edition of a work which advocates local and constitutional medication, and the application of electrolysis and cataphoresis, in place of operation, for the diseases mentioned in the title. While the author deplores the current tendency to surgical interference, he is careful to insist on the necessity of training and instruction in the use of electrical therapeutics. The book is the outcome of more than twenty years of clinical experience. The present edition describes three important and novel modes of treatment.

S. P. WORTHING.

The Greek Verb Graphically, by W. SIHLER, A. M. *The German Noun: Its Difficulties Simplified*, by W. SIHLER. Decorah, Iowa.

The first of these pamphlets represents an attempt to teach the Greek conjugation graphically, by means of simple geometrical figures. The author argues that, at the time when Greek is learned, the eye has already received training from drawing and geometry, while visual memory is also better than auditory. Some hundred diagrams, printed in two colors, together with a page of endings, provide the necessary material.

The second pamphlet gives rules for German genders and plurals taken from Kaeding's *Häufigkeitswörterbuch*, with some mnemonic helps for irregular forms, materials for exercise, and a vocabulary.

G. PARTEN.

Ueber einige Berührungstäuschungen. Von F. KIESOW, 1907.

Ueber einige Streitpunkte auf dem Gebiete des Geschmacks. Von F. KIESOW, 1907.

Sulla presenza di organi del gusto nella parte laringea della faringe, nel tratto cervicale dell' esofago e nel palato duro del feto umano. Nota del Dott. M. PONZO, 1907.

Sulla presenza di calici gustativi in alcune parti della retrobocca e nella parte nasale della faringe del feto umano. M. PONZO, 1905.

Intorno alla presenza di organi gustativi sulla faccia inferiore della lingua del feto umano. M. PONZO, 1907.

Contributo al problema della localizzazione delle sensazioni. Di M. PONZO, 1905.

(1) Reprint from *Arch. f. d. ges. Psychol.*, X. Deals in the main with an illusion of touch; when the acuity of localization is determined by Weber's method (retouching by the observer of a point already touched by the experimenter), it often appears as if the first touch (the cue to localization) were given not by the experimenter but by the observer himself. The paper describes various modifications and seeks to determine the conditions of the illusion. (2) Reprint from *Zeits. f. Psychol.*, XLV. Review of W. Sternberg, *Geschmack und Geruch*. (3), (5) Reprints from *Anatomischer Anzeiger*, XXXI, XXX; (4) reprint from *Giornale della reale Acc. di Med. di Torino*, XI; describing and figuring taste-beakers in the regions indicated in the titles. (6) Reprint from *Atti del V. Congresso intern. di Psicologia*. In cases of 'lingual inversion' (reversion of the tongue from left to right), localization is accurate under the abnormal circumstances, the direction of movement is subjectively reversed, and a form of Aristotle's experiment is possible. Similar experiences occur on the pinna of the ear, scrotum and penis. W. JENKINS.

Transactions of the Congress of American Physicians and Surgeons. Seventh triennial session held at Washington, D. C., May 7, 8, and 9. Published by the Congress, New Haven, Conn., 1907. pp. 164.

The papers here printed are grouped under two heads: first, the historical development and relative value of laboratory and clinical methods in diagnosis, and the four papers here printed are prepared by Drs. Osler, Barker, Stengal and Cabot; the second group is on the comparative value of medical and surgical treatment of the immediate and remote results of ulcer of the stomach by Drs. Muser, Stockton, Mayo and Munro. The address of the president, Dr. R. H. Fitz, treats of the borderland of medicine and surgery.

Diseases of the Heart and Aorta, by THOMAS E. SATTERTHWAITE. E. R. Pelton, New York, 1905. pp. 304.

The author tries here to place before the general practitioner a brief, simple, but practical presentment of cardiac and aortic affections from his standpoint of his personal experience. Over a hundred cases from his records are given at length. Little space is devoted to the anatomy or surgery of the heart, to its parasites, or to congenital anomalies or tumors. The book is based on a series of articles which originally appeared in medical journals, and appears to be a very valuable collection of precepts.

Wissenschaftliche Beilage zum zwanzigsten Jahresbericht (1907) der Philosophischen Gesellschaft an der Universität zu Wien. Johann Ambrosius Barth., Leipzig, 1908. pp. 42.

Wege und Ziele psychiatrischer Forschung, by R. GAUPP. H. Laupp-'sche Buchhandlung, Tübingen, 1907. pp. 28.

Ueber die Nachwirkungen der Vorstellungen, by W. SCHAEFER. O. Kindt, Giessen, 1904. pp. 44.

Die pseudomotorische Funktion der Hirnrinde, von RICHARD STERN. Franz Deuticke, Leipzig, 1905. pp. 27.

Faith in Man, by GUSTAV SPILLER. Macmillan Co., New York, 1908. pp. 190.

Travaux du Laboratoire de Recherches Biologiques de l'Université de Madrid, Tome VI, fascicule 3°, Août, 1908. Edited by S. Ramón y Cajal. Nicholas Moya, Madrid, 1908. pp. 97-160.

Pragmatism, by PAUL CARUS. The Open Court Pub. Co., Chicago, 1908. (Rep. from The Monist, July, 1908. pp. 321-362.)

Index of 1180 Post-Mortems of the Insane, by H. J. SOMMER. Published by the Board of Trustees of the State Hospital for the Insane, Norristown, Pa., 1908. 316 p.

L'évolution du Psychisme, par HENRI PIÉRON. Éditions de la Revue du Mois, Paris, 1908. 24 p.

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NAMES OF AUTHORS

(The names of those who have contributed original matter are printed in SMALL CAPITALS.)

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